UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON THE NATURAL SYSTEM

UNSATURATED ZONE FLUID FLOW AND RADIONUCLIDE TRANSPORT

March 9, 2004

Crowne Plaza Hotel
4255 South Paradise Road
Las Vegas, Nevada 89109

NWTRB BOARD MEMBERS PRESENT

Dr. Daniel B. Bullen
Dr. Thure Cerling, Chair, Afternoon Session
Dr. Ronald Latanision
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek, Chair, Panel on the Natural System

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
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Dr. David Diodato
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CONSULTANTS

Dr. Frank Schwartz, Ohio State University
Dr. Rien van Genuchten, USDA/ARS

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(Cont.)

P R O C E E D I N G S

8:00 a.m.

PARIZEK: Good morning. It is my pleasure to welcome you to the meeting of the Nuclear Waste Technical Review Board Panel on the Natural System. I am Richard Parizek, and I am the Chair of the Panel. As many of you know, the Board was created in 1987 in amendments to the Nuclear Waste Policy Act. Congress established the Board as an independent federal agency to evaluate the technical and scientific validity of activities of the Secretary of Energy related to the disposal of spent nuclear fuel and defense high-level nuclear waste.

By law, the Board reports its findings, conclusions and recommendations at least twice a year to Congress and to the Secretary of Energy. The President appoints Board members from a list of nominees submitted by the National Academy of Sciences and designates a member to serve as Chair of the Board. By law, as well as by design, the Board is a multi-disciplinary group with a range of expertise. A full Board consists of eleven members. There are three vacancies at this point.

Now, let me introduce the members of the Panel on the Natural System, and other Board members and consultants who are here today. Let me also remind you, before I do, that all Board members serve in a part-time capacity. We all have day jobs. In my case, I am a professor of Geology and Geoenvironmental Engineering at Penn State, and also President of Richard R. Parizek and Associates, Consulting Hydrologists and Environmental Geologists. My area of expertise include hydrogeology and environmental geology.

Board members in attendance at Dan Bullen. Raise your hand. Thure Cerling, Ron Latanision, Priscilla Nelson, and myself. With the exception of Ron, all are members of the Panel on the Natural System.

Dan is from the great state of Iowa, and is on leave of absence from the Mechanical Engineering Department at Iowa State. He joined the office in Chicago of Exponent at the beginning of this month. His area of expertise include nuclear engineering, performance assessment, modeling, and materials science. Dan chairs the Board's Panel on Repository System Performance and Integration.

Thure Cerling is Distinguished Professor of Geology and Geophysics and Distinguished Professor of Biology at the University of Utah in Salt Lake City. He is a geochemist, with a particular interest in applying geochemistry to a wide range of geological, climatological, and anthropological studies.

Ron Latanision chairs the Board's Panel on Engineered System, and is a principal at the venturing consulting firm, Exponent, a Professor Emeritus of Nuclear Engineering and Materials Science and Engineering at MIT, and last, but certainly not least, a graduate of a well-known state university in central Pennsylvania. His interests of expertise include materials processing, the corrosion of metals and other materials in aqueous and non-aqueous environments.

Priscilla Nelson is Senior Advisor to the Directorate for Engineering at the National Science Foundation. Her areas of expertise include rock engineering and underground construction.

We are also pleased to have two consultants, Frank Schwartz and Rien van Genuchten, raise your hands, with us today. Frank Schwartz is an Ohio Eminent Scholar in Hydrogeology at the Ohio State University, and has served other groups interested in independent scientific evaluations of Yucca Mountain hydrogeology. His areas of expertise include fluid flow, solute transport, and basin-scale hydrogeologic analysis. Many of you would know his books. He co-authored several books, and a number of publications.

Dr. Rien van Genuchten is a Research Soil Physicist at the U.S. Department of Agriculture Research Service in Riverside, California. He is an expert on analytical and numerical mathematical descriptions of unsaturated zone fluid flow and solute transport processes.

Welcome both of our consultants. He's the father of variable, you've heard of van Genuchten, variables, you know, it's nice to have things named after you.

At the side of the room, and on the right-hand side from your perspective, are the staff of the Board. I expect the staff will be actively involved in our deliberations today, and, so, you will certainly hear from them as we proceed. Thank you for your efforts.

Bill Barnard, the Board's Executive Director, is sitting on my right. On the left, okay.

Before we turn to today's meeting, the Board would like to announce a change in the leadership of the Panel on the Waste Management System. Much of the Panel's activity for the foreseeable future will be related to transportation of spent fuel and high-level waste, and Mark Abkowitz is the Board's expert in this area. Many of you would have met him in the January meeting, also held in this room. Accordingly, the Board has decided that it makes sense for Mark to chair this panel. The Board thanks Norm Christensen for his efforts in chairing the panel over the past couple of years.

The theme of this meeting is hydrogeology of the natural system, specifically including aspect of the natural system related to fluid flow and radionuclide transport. In

May of 2002, when the Board first met OCRWM director, Dr. Margaret Chu, she expressed an interest in further evaluation of the potential performance of the natural systems, and identified the saturated zone as an area of interest. The Board has developed a list of six issues related to the performance of the natural system. That list is projected on the screen in front of you.

What is the median travel time of a molecule of water from the repository horizon at Yucca Mountain to the repository regulatory boundary?

How might travel time change for a radionuclide in the water, considering all factors relevant to radionuclide transport? Are all of the factors equally likely?

Are the DOE's radionuclide transport time estimates conservative, realistic, or optimistic?

What is the technical basis for these estimates? What is the Board's assessment of the technical validity of the technical basis? What can be done to improve the technical basis of the DOE estimates?

How much could the technical basis be improved by 2010 if the DOE pursues a rigorous scientific program?

Each of the talks to be presented today and tomorrow help to evaluate these issues. Today, we will focus on the unsaturated zone and climate, and tomorrow, we will address the saturated zone. Tomorrow's meeting will include a roundtable discussion of panelists in the afternoon. We look forward to an opportunity to engage in further discussions and reactions to what we hear over the course of today and tomorrow.

This morning, we will begin with a presentation from Eric McDonald of the Desert Research Institute, about the deposition of sediments in the desert that result from climate change. That should give us insight into not only how often climate has changed in the past, but also the character of the sediments, and how they might affect fluid flow and radionuclide transport.

That talk will be followed by a presentation by another DRI researcher, Saxon Sharpe, who some of you have heard make a presentation here approximately a year ago, and he will describe the technical basis for the DOE's understanding of present and future climate states. Understanding climate is important for understanding precipitation, a significant factor in fluid flow and radionuclide transport.

Following that talk, James Paces will present analyses and interpretation of minerals collected inside of Yucca Mountain. And, you've heard from Jim in the past, and we look forward to these new presentations from all speakers.

The last presentation of this morning will be given by Alan Flint of the U.S. Geological Survey describing past and present theories of how water moves in the unsaturated zone of Yucca Mountain.

This is a Panel meeting, and not a meeting of the full Board. Panel meetings provide an opportunity for the Board to focus on in-depth discussions of particular issues. The Board deeply values public participation, so we have given the public a variety of ways to comment during this meeting. We have set aside time for public comments before lunch, and then again at the end of the afternoon. The period before lunch is intended for people who, for one reason or another, cannot remain until the public comment period at the end of the day. Some people may simply not be able to stay for the entire program.

Is there anybody here who wishes to speak that will not be able to remain until 5:20? I see no hands. If you would like to speak during the afternoon session, please add your name to the sign-up sheets for public comment at the registration table where Linda Coultry and Alvina Hayes are located. And perhaps they can raise their hand out here in the back. So, please add your questions to their list. If you ladies would just raise your hand, as you did, they'll know where to find you. But that's normally the back table by the entry door.

Most of you who have attended our meetings know that we try very hard to accommodate everyone, but as you can see, as usual, we have a tight agenda. Depending on the number of people who wish to speak, we may find it necessary to limit the time of those presenters. As always, we welcome your comments, including written comments for the record.

Board and Panel meetings are spontaneous by design. Board members speak quite frankly and openly about their opinions. But, I have to emphasize that when we speak, that we speak on our own opinions, and we're not speaking on behalf of the Board. When we do articulate a Board position, we will, of course, make that very clear. Board positions are stated in letters and reports, and are available on the Board's web site.

Before we begin, I would request that cell phones be turned off. We don't want anyone to have to suffer the embarrassment of having the rest of us start pointing and possibly noting their name for the record. So, please, silence the cell phones.

So, having made that reminder, we're now ready to introduce our first speaker, Eric McDonald. He is a soil scientist and geomorphologist with the Desert Research Institute. Eric, it's a pleasure to have you with us. Welcome, and the floor is open.

MCDONALD: I have a power point presentation. How are we doing this? Can everybody hear me all right? Yes?

Just to sort of fill the dead time here, this is the first time I've spoken before, given a presentation for this Review Panel. My interests in deserts is broad, but one of my favorite topics is the history of alluvial fans, and what I'm going to show during this presentation is sort of some general aspects of alluvial fans. This sort of sets the stage as to some of the general characteristics of the basal sediments. The soil is on top. The main part of my talk will be looking at how alluvial fans sort of record climate change, or put in other terms, reasonable climate change clearly draw--major alluvial fan depositions. That's what I'm going to try to show during most of the talk.

Earlier, I sort of call myself a geomorphologist, and I'm on the desert--most of the land forms you see I think record events that we can't really explain by modern day processes, and this includes climate change and how that impacts the landscape. So, hopefully, I'll keep this talk pretty general, and use this just for basic background, alluvial fans.

I was asked to sort of talk about a variety of things. The first one is is alluvial fans contain a range of sediments from cobbles to clays. Basically, they are very mixed sort of range or particle sizes, and they also are capped by soils. I'll talk about some of the basic types of soils, or quality of soils.

Alluvial fans can be stacked on top of one another in basins. Basically, basins are fixed--of alluvial fan deposits, and this could be seen in a variety of stratigraphic exposures, and I'll talk just a little bit about that. What I'm going to really focus most of my time on is the idea of climate change is frequent and regular, and drives alluvial fan and lacustrine deposition across the deserts. In other words, in the case of alluvial fans, major periods of alluvial fans are indeed driven by changes in climate.

Outline. So, we'll start of first, general character of alluvial fan deposits, look at some surface and buried soils, and a little bit on control on infiltration. Part of my work with alluvial fans in soils is how the soils control surface water hydrology, both infiltration and runoff, and I think this, in part, comes back to the purposes of review for today.

Deposition of alluvial fans are regional events. I'm going to show some data we have that these things indeed occur at intervals across a region, and look at a detailed record in the last 25 years of events, fan deposition, and look at the larger record over the last 85,000, 75,000 years. And, then, try to make the point that fan deposition is indeed related to some aspect of climate change.

The work I'm going to be talking about is largely the East Mojave. Here's the test site up here. I've only done some work at the test site. Most of my work is in the Snore and Mojave Deserts. But, the evidence I will talk about today will clearly apply to the test site areas of this Fortymile Wash. This is also part of the Great Basin as far as this part of the Mojave right here, very similar in many ways to the test site environment.

This is a satellite photo of the typical desert sort of Piedmont or bajada. Here's the bounds right here. Off there is large pockets of dunes, and this is referred to as the Piedmont or the bajada. And what's really important is that this surface here, which looks pretty simple, is actually a very complex mosaic of different age deposits with different types of soils. It's like a big jigsaw puzzle.

In this case, the different colors are different aged units. The yellows are basically young units, and the blues are units older and really near. So, you have this sort of puzzle mosaic of very different types of fan deposits at the surface.

Sort of a very simple schematic diagram, alluvial fan setting. Here is a diagram of the mountain front, usually some sort of range fault down the mountain front. This would be, say, the active channel shown here in blue.

Fan deposits come out of the mountain, basically sort of fill in this basin, and this just shows the idea that we do indeed have a sequential stack of buried deposits, alluvial fan deposits.

Throughout the talk, I'll often refer to the proximal fan and distal fan. Proximal fan is the environment at the fan apex right from the mountain front, where the sediments first leave the mountain basin, and are deposited into the basin. And, then, we have these distal fan environments. Generally, proximal fans are steeper gradients, three to five to ten degrees. Distal fans usually three to five degrees as far as the actual gradient.

These alluvial fans, there is a very profound change in particle size from the mountain front through the fan to the basins. This is a very simple diagram. Proximal fans, lots of boulder and deposits, lots of free flows, very coarse, poorly sort of deposits, as you go towards the distal fan, due to changes in energy of transport, mostly sand, gravels. So, we see a change from coarse deposits on the mountain front, and finer deposits as we get away from the mountain front towards the valley bottom. This same record will be preserved in the basin sediments below ground level.

Some photographs just to highlight this point. On the left here, this is corner, proximal fan deposit, here's the ladder for scale, lots of boulders many meters in diameter, poorly sorted, lots of debris flows. These things were stacked. Here's a layer, layer, layer and layer. By comparison, here's the distal fan deposit, here's also a meter scale. Lots of sand, lots of gravel. So, a very profound difference in particle size between the proximal setting and distal fan setting.

This is a mosaic map of the different deposit. I'm going to go over this again. This is very typical for most Piedmont in the Great Basin. Yellow, some light browns here are deposits less than 10,000 years. There's quite a few of those. Deposits here in the green and the purple, between about 10,000 to 150,000. And, we have a record of quite a few alluvial fan deposits greater than 500,000 years, and they're shown here in blue. Again, we have this mosaic of very different age deposits exposed at the surface.

What's really important also is that the type of soil that forms on these deposits will vary as a function of surface age and the type of parent material. In this case, we have limestone, volcanics and granites and quartz monzanite side by side, and we can look at the different types of soils that perform these environments. These things are simple block diagrams. This is the soil depth. These are just little cartoons, basic types of soils. Here's the limestone. The white here, this shows strong accumulation of calcium carbonate, not too surprising in the fact that it's limestone. We sort of see mixtures that have more siliceous materials, such as quartz and quartzites and granites, and so on and so forth, sandstones. We get lots of calcium carbonate accumulation. We also get the accumulation of sodium chloride, called Color B horizons or clay B horizons.

As you go more into the granite materials, less carbonate and a lot more in the way of clay rich horizon. So, across these alluvial fans, we'll have a wide range of soil types, both in terms of carbonate and in clay content.

An example. This is the typical soil you find in the Holocene age deposit, very weak development, usually less than 10,000 years, very sandy texture, limited horizonation. Basically, just the actual primary sediments, loose matrix. These soils have very high infiltration.

By comparison, on the same setting, you can have lots of deposits, soils form on these old deposits, old in this case being greater than 10,000 years. Also, clay right here shown by the orange color, lots of accumulation of calcium carbonate by the white here. These soils, old deposits, clay-rich texture, very complex horizonation, that is, a very stratified sequence, different types of horizons, often cemented matrix, matrix cemented by calcium carbonate or silica. These soils have very, very low surface infiltration.

Another example of soils, young soil, very weak development, and the common setting on soils in these Piedmonts, clay-rich here, lots of clay right here, some carbonate. In some cases soil matrixes, they are almost completely cemented by secondary calcium carbonate. So, a wide range of soil types on these alluvial fan surfaces.

Another key point is that alluvial fan surfaces are natural dust traps. A very common feature in the desert is wind-blown, and we have shown and we found that over the years, over many millennia, these soils will just accumulate vast quantities of silt and clay from the dust at the soil surface. So, it used to be a very high concentration of soil and dust here, and this also dries desert pavements or these tightly fitting mosaic of class, the surface, very common alluvial fans. And all this area represents this long-term accumulation of desert dust at the surface.

Buried soils, alluvial fans. They do occur. Here's a couple of examples. These are two buried soils here, this main deposit, one down here. In my experience, most buried soils are usually the remains of these carbonate rich horizons. The other horizon has been stripped off, so, we have these sort of buried petrocalcic horizons, horizons cemented by calcium carbonate.

I think a couple key points, this is based on my own personal experience. Buried soils are often called Paleosols do occur in fan deposits. They are more likely to occur in the distal fan environment. This is because this is an environment that's largely characterized by aggredation, so deposits can be preserved.

In the proximal fan environment, the older deposits are often buried, and are often eroded. And, so, you have a very poor preservation of the soils. So, buried soils, more likely in distal environments; less likely in proximal. And more importantly, also is the buried soils are going to be discontinuous. They're not likely to be preserved as a continuous layer across the landscape. So, the record of buried soils in alluvial deposits can be very spotty.

A little more information on soils. A key thing about soils is that soils build over time, and you have an increase in silt and clay. So, a soils get older, you have more silt and clay, also more carbonate. This is depth profiles. This is down through the soil this way, and this is showing mass of silt plus clay, sort of normalizes, removes the gravel. These are different pan materials. This is basically a thousand year old deposit we're starting with. This is a small amount of silt plus clay. This would be all paramaterial.

In 10,000 years, if you look near the top of the profile here, this is a definite accumulation of silt and clay, and this is from dust, not necessarily weather, like mostly from the accumulation of the desert dust, and 150,000, 130,000 years, these are very strong increase in silt plus clay, especially near the surface. So, as time goes on, we see this very strong accumulation of silt and clay in these soils, desert soils, especially near the surface. This is very typical for most alluvial fans, and it occurs on almost all paramaterials, including vernix and limestones, it's pretty much the same. So, a strong accumulation of silt and clay over time in the near surface environment.

This is really important. It has a huge impact on the infiltration, surface infiltration. This is just some double ring petrometer measurements done a few years ago. Millimeters of water, this is infiltration time. Active wash, just basically loose sand and gravel, very, very fast 40, 60 centimeters of infiltration. What's really interesting is that this late Holocene surface is about a thousand years old. This is a very small accumulation of silt and clay from desert dust, maybe a centimeter at the top of the soil. It has a very profound impact on infiltration. On the older soils, we have developed what's called a vesicular A horizon. This often forms the desert pavement. It's a very silt and clay rich horizon, about 60 meters thick right at the very top of the soil. It has a very, very strong control on infiltration.

So, the older alluvial fans, the soils in the older alluvial fans are more likely to permit runoff into nearby channels and have less water moving down through the soils. So, the soil environment of the fans will have a very strong impact on the surface hydrology, which also means they have an impact on the water as it percolates through the soil.

All right, let's go on to looking at the alluvial fan record in the last 85,000 years. This sort of multi-color messy diagram, this is a regional correlation chart. This is alluvial fan record from the Providence Mountain I've been talking about. That's the one that had the satellite photo. This would be the Silver Lake or the Soda Mountain near Baker, California, and this is alluvial fans and volcanic deposits in Cima. The yellow is the Eolian or sand sheets, the sort of brown are fan deposits, and the orange are volcanic deposits.

The blue here shows correlations across the region. This first one here is that we can use age control, in this case, if red simulated luminescence, cosmogenic brillium 10 radiocarbon, potassium argon, and cosmogenic helium 3, to use age controls to start correlating these deposits. What we're trying to do is build this regional structure for framework of deposits across the region. We're trying to link these deposits, A and B related in time as far as periods of deposition. From here on up, this is basic layers of pleistocene, through Holocene, and we do have some older alluvial records dating back to about 85,000 years as far as sand deposits.

So, we can use age control in part to start linking these deposits together across the region. What we can do also is use salt formation to help link these soils, link these thoughts together. We can use the soils to reinforce the age control. So, again, we use the soils to sort of help build this framework. There are many ways to show soil data.

What we often do is we use what's called a soil development index, and this is just basically an index. We take different types of soil properties, morphology, the structure and the color, and so on and so forth. We can easily apply a value to it. The higher the number, the older the soil, the strong the degree of soil formation. And, we can play games like link these things together. The key thing here is these are the three different sequences, Providence Mountain, Silver Lake or Soda Mountains, the Cima, and we can use the soils to show that these deposits, alluvial fan deposits, are indeed correlative across the region. So, we use the soils and the age control to form the stratigraphic framework.

All right, let's look at fan deposition is related to climate change. There clearly is a record of alluvial climate change in the Great Basin of the Mojave and (inaudible) Deserts. Saxon's talk will actually provide more detail. We know climate change is rapid. We know it's frequent. We know it happens in deserts, and it has a profound impact on the alluvial record that we see.

This is a schematic of the record from Lake Mojave. This is near Baker, California. This is probably the most important record we have in the Mojave Desert. We have two major lake events during the last ice age, the last major pluvial, Lake 1 and Lake 2, by some intermedial lakes. So, a lake was going up and down, it was pretty sporadic. We also have some clear evidence of lakes during the Holocene, the last 10,000 years, actually, the last 8,000 years, at least four different lakes. So, again, the lakes here represent periods of climate change, and I'll show later these represent periods of wetter climate across the region.

So, here's our climate record. Here's the alluvial fan record from the Providence Mountains. The yellow, these are periods of sand sheets or Eolian deposition. The brown here would be alluvial fans. And, we have several fans during the last 14,000 years. The biggest one at this time period is Qf5, and it's clearly tied into a period of high lake sand and diminishing lake during the Plubial Lake record. So, we see fans being tied back into part of the pluvial record. The same thing in the Holocene here. We have some fans that seem to correlate with some of these short but important Holocene lake sands

What's really important is that we can see this same record in other mountain fronts across the east Mojave. This would be the Silver Lake/Soda Mountains, this is near Baker. We have a very similar record as far as alluvial fans, and periods of sand sheets. The key thing here is that across the region, we're starting to see very similar periods of alluvial fan deposition. They're occurring during these brief periods of time, and they seem to be occurring during the same time intervals across the basin.

This is really important because these are two very, very different environments, as I'll show next. This is sort of a basic comparison for the Providence and the Soda Mountains. This would be the largest basin we find in the Providence, the largest basin we find in the Soda. This is all in the basin. This is a kilometer by kilometer scale for comparison.

The other key thing is that if we look at the drainage profiles in the basins, a huge difference in elevations and environments. This would be the gradient for the Providence, above 1,000 meters, or 2,000 meters, and here is the drainage for this basin, the Soda, well less than 300 meters.

What's really important is that these are two completely different environments. Providence, high elevation, semi-arid, sub-humid, continuous vegetation. Vegetation covers as far as today. Soda Mountains, very low elevation, very arid, almost hyperarid, sparse vegetation cover. These two different mountain fronts, mountain basins, were depositing alluvial fans from the same time period. To me, this represents how climate change is driving alluvial fans, and not some sort of material mechanism like complex response to internal factors.

If we have alluvial fans being deposited from very different environmental settings, something else is driving it besides internal factors. Again, the external factor would be some part of climate change.

We can also see this sort of propagation across different levels of the tectonic activity. This is a very simple tectonic map of Southern California. Right here, it's very high tectonic activity. Here's the San Andreas and the Garlock, and a series of mountain fronts that are very active tectonically. This would be the Silver Lake/Soda Mountain front right here.

We can compare that alluvial fan record with the Providence and the Cima. These are basically areas of very low tectonic activity. So, again, the point here is that we're seeing regional deposition across different geomorphic settings as far as environment, and across different tectonic activity. So, the type of tectonic activity does not control these discrete periods of alluvial fan deposition. These are regional-wide events.

So, if you look at the--bring this back a little bit. This is the record I showed earlier. This is about 25,000 years. We have recent age control on this what we call the Qf3. This would be a fan deposit that we're finding across the region. This is a very large fan, interval of fan deposition, and occurred about 65 to 75,000 years ago. If we compared this to most, this is sort of a compilation of most alluvial lake records in the Great Basin of Mojave, there's plenty of evidence for a lake stand across the region about 65 to 75,000 years ago. So, again, we see a period of wetter climate, and we see a fan associated with that wetter climate.

So, the point here being that the alluvial fan is clearly responding to climate change, in this case, some wetter climate, and the recordings are intervals of wetter climate.

Now, how climate change impacts alluvial fan deposition, there are still many questions. There's a sequence of events regarding vegetation change, regarding storm intensities, storm size, that we haven't quite figured out. But, I'll just simply leave with this. We know that during these periods of wetter climate, it there was indeed wetter across the basin. This is a very simple way of showing this. There are many better ways to do this. This is an elevation of weather stations across the basin, different elevations. This is annual precipitation, and this is basically about 60 years of historic weather data.

The red line here is the historic mean, and this would be their typical year, and the blue line up here, these are flood years, or years of El Nino type weather activity. In this case, this is years in which the Mojave River actually flooded, putting water into the Silver and Soda Lake Basins. This is a rare event, but this is when we have a large increase of frontal storm activity. The key point here is across the region, there's almost a doubling or tripling of the amount of rainfall that you look at. So, during these pluvial periods, we also use this as a record of the climate mechanism driving these pluvial periods in the Mojave Basin.

So, we clearly see an increase in moisture across the region when we have these pluvial periods. So, again, how this drives alluvial fan deposition, we're still not 100 per cent sure, but we do know that when you have wetter environment, you do have these periods of alluvial fan deposition across the region.

This last slide here is going to highlight this point. The record developed in the Mojave Desert right now is that the lacustrine record, and to some degree, the alluvial fan record, reflects this period of change in storm tracks. During the pluvial periods, the (inaudible) drops to the south, and most of the storms are frontal storms, funnelled through Southern California. Whereas, say, historically or typically, most of the storm tracks lie well to the north.

So, clearly, we see this period of alluvial fan activity during periods when we know there was increased wetter climate across the Mojave Desert. This would also apply to the Great Basin Desert.

Let me summarize this. Alluvial fans contain a range of sediments, coarse grain, cobbling near the mountain front. Internal particle size decreases down fan, with more silts, clays and sands in the distal fan environment. Soil development increases with surface age, carbonate accumulation, silica accumulation, silt and clay from dust. Infiltration decreases with surface age, a huge impact on the infiltration and the resulting hydrology of the surface.

Alluvial fans can be stacked on top of one another. These basins contain a series of different alluvial fan events. These fans do contain buried soils, but my experience has been that the best preservation of buried soils are in distal fan environments, with preservation being discontinuous.

And, finally, the climate change is frequent and clearly drives alluvial fan activity, along with the lacustrine activity. The key point here being that the alluvial fan record we see is related in some aspect to climate change. We see discrete periods of region-wide alluvial fan deposition, across all basins, across at different range of tectonic activity. Alluvial fan deposition is clearly related to some aspect of climate change. Exactly how that happens, we don't know. There's a variety of ideas, but clearly, climate change is driving these major periods of alluvial fan deposition.

Based on the record we have in the East Mojave, at least five major periods of fan deposition in the last 75,000 years, there are probably more, but those are the ones that we can reasonably correlate right now. And, there's still, like I said earlier, big questions on how this happens. There's clearly links between regional climate change and regional periods of alluvial fan deposition.

And, with that, I'll take any questions. Thank you.

PARIZEK: Thank you very much. When the viewgraphs didn't come up right away, I might have commented on why all of this might be important to the Yucca Mountain Project. Surely, you've given us an understanding of a variety of conditions that might occur through time, and how that drives fan development and sand down cutting.

One question is how do we get a canyon cutting stage added to a fan? When do we fill a canyon in? So, we look at Fortymile Canyon, Fortymile Wash, versus the distal end, how does that evolve through this? And, given the soils that you show, I mean, there in the field trip where you illustrate this, it's really convincing evidence that it takes skill, it takes knowledge, but when you do that, you have this permeability contrast affecting infiltration, but also the possibility of flow in the saturated zone. How many soils could we have in a fan like Fortymile Wash at depth, and down at the saturated zone? How do we know we have them by drilling? We now have a sonic core capability that might be a way to do this. The first core starts at the water table, however, it kind of ignores a lot of the shallow material. There's a series of questions here that would be helpful to understand, because this is very relevant to how you treat modeling and water flow and transport in a fan complex.

MCDONALD: Well, let me try to answer that second question. I have worked on projects. We've looked at buried soils and cores. It's very difficult. When I look at soils in the field, I need a meter, 2, 3 meters to really get a sense of what that soil is all about, because the soil variability, when you look at a core that might be two inches or four inches across, that's really a challenge.

These alluvial fan basins, clearly are buried soils, especially like I said, in the distal fan environment, that would be the geomorphic environment most likely to find buried soils. So, I'm taking soil pits in the distal fan environment. I often encounter buried soils, even in the soil pits. They do occur out there.

Given that sort of mosaic pattern, alluvial fans, given the fact that you do have this sort of combination of aggradation and degradation, preservation is going to be very, very spotty in the alluvial fans as far as any one soil, alluvial fan surface being preserved, intact in a buried environment. So, I can almost visualize these sort of pockets or stretches of soils here and there. So, it is sort of hit and miss as far as drilling.

I would say, just thinking off the top of my head, that it would probably take more than one drill core over some interval, you know, over 100 meters, 200 meters, whatever, to be able to pick up buried soils, because it is a spotty record.

And, my experience also is that in most of these cases, most environments, you're only preserving the strongest part of the soil. It may be clear (inaudible) that part of the soil submitted with calcium carbonate. In some cases, that may only be a few decimeters thick. So, it may be a very difficult record to pull out of these basin environments, but it should be there. I think that's the big question, what is the, if you look at this in sort of a three dimensional sense, how many soils could be buried, how large an area do they cover, so on and so forth.

MR. PARIZEK: But, there's surely an episodic evidence that you show us from the lake levels, plus also fans over a broad area in the Mojave Desert, and I think that's interesting because, say, for the Fortymile Wash area, we're likely to have had more complicated than perhaps a simple rendition of it, and the question is what does that mean to perhaps model development, and the heterogeneous nature of the deposit you show us also has allowed significance to the model.

MCDONALD: I just think that especially in a place as big as Fortymile Wash, when you get to those distal environments, there's such a huge fan system and drainage system and terraces, and what not, that just thinking about the complexity of how much could be preserved, it's actually immensely quite a challenge. Clearly, there's got to be something there.

PARIZEK: Ron?

LATANISION: Latanision, Board.

Let me preface my question by pointing out that I'm a metallurgist who has had I think, Richard, two courses in geology when I was a student at that wonderful campus in the Nitany Valley of Pennsylvania.

But I'm interested in, let's see, there's no number, the slide that showed soil development. I'm wondering what the--I think we passed it--what is it that's actually quantified in terms of the morphology? And, I ask this question because in terms of the solid state, we teach our students, or I have taught my students, I should say in the past tense, the importance of the relationship between the processing of the solid, its structure, or in this case, perhaps morphology, and ultimately its properties. And, so I'm just wondering what characteristic it is that's identified in a soil development index, and whether it is a manifestation of the, let's say, the rate of deposition of alluvial material or just what it actually characterizes.

MCDONALD: Right, Those are two big questions. Let me go with the index. When we describe soils in the field, there's a wide range of properties we describe. Basically separate the soil in the horizon in discrete layers. We describe the color, the structure, the type of carbonate coatings, the type of clay coatings. There's a long list of morphologic properties in the soil we describe.

What the index does, it simply takes all those different types of soil properties, and we normalize those against what we think is the strongest property you could find in that environment, and we basically take all those properties and throw them together as a single number. So, we're taking a wide range of morphologic properties, and playing some games, come up with a single number that could, for example, sort of represent that profile. We can also look at numbers for the horizon as a function of different types of properties.

In most cases, we use the index, increasing soil formation leads to a greater development of morphologic properties, a greater type and a greater degree development and a greater range of morphologic properties, like is reflected in the index. The soils get deeper, and that's also reflected in the index. The final number is a combination of the depth of the soil, along with the overall summation of types of morphologic properties.

So, in short, the index is sort of a way to very simply show the degree of soil formation. The higher the number, the more greater variety, degree of development of morphologic properties.

LATANISION: Is there a way of interpreting the index in the context of infiltration rate?

MCDONALD: You could. There's two ways to do it. One is in these environments, generally speaking, the older the soil, the stronger the development, to lower the infiltration.

LATANISION: Okay.

MCDONALD: Basically, what you're talking about is the higher content of clay and silt, greater degree of structure and greater degree of calcium carbonate accumulation. Things are going to slow down in the infiltration and transmission of water.

LATANISION: So, would a high index typically have a low infiltration rate?

MCDONALD: Typically, to a point. On the older soils, what makes this really fun is that we know in the desert a good question I can--we often ask is how come we don't find well developed, intact soil in the Mojave Desert. Because of the change in infiltration. As the soils become better and better developed, and the infiltration decreases, we've reached a point where the soils begin to self-destruct, as you decrease infiltration, you produce more runoff, which leads to surface erosion. So, it's sort of a strange cycle in the older soils, where you might be removing some of the horizons that can best limit infiltration. But, generally speaking, it's sort of like a meter thick petro-calcific horizon, lots of calcium carbonate, it's still going to decrease infiltration.

LATANISION: If we could turn to the slide that showed infiltration? It's a few prior to this one. This is interesting to me. You made the comment that if there's a thin layer of clay, for example, on the surface, it will affect the infiltration rate dramatically.

MCDONALD: Right.

LATANISION: And, that leads me to an analog again with the solid state in which we often deposit thin layers of various materials, for example, in semi-conductors, we're likely to dope a semi-conductor with a metaloid element, or some such, and that changes properties dramatically. I'm wondering if the same might be true in the case of geological structures or perhaps if the scale is too big for this to be practical, but the sort of wild eyed thought I'm having here is whether or not you can actually conceive of tailoring soils by artificially introducing into the surface constituents that might have the effect that clay does here in modifying the infiltration rates, and whether that sort of artificial processing might actually be of some value in a geologic sense.

MCDONALD: That's really a good idea. I would say if you have some alluvial units somewhere at depth, and you wanted to, say, inject carbonate or clay into it, clearly we have to change hydrological properties. Certainly, that would clearly have an impact when it comes to soil environment. The other key part is soils, not just the fact you've got silt and clay, but also it has to do with the development of soil structure, which controls the pore size distribution, and especially also macroporosity, and that's really more of a soil function. So, the question would be if you injected, say, a buried alluvial unit, you'd certainly have the particle size change, but you also have some of the corresponding changes as far as the porosity, and what not. But, I mean, just generally speaking, if you were to inject a finer grade material into a coarser grain buried deposit, it would have to have an impact on the flow of water.

LATANISION: Yeah, that's what I'm thinking.

MCDONALD: I never thought about that, but it should. I mean, I'm trying to do the same thing in the surface. I'm trying to develop a way to recreate these desert pavements on the surface, for the same reason, because they control the ecology, they control the infiltration runoff, they stabilize the surface. They're being destroyed in the desert. It's the same idea, trying to artificially create this sort of fine grained unit. That's really an intriguing question.

PARIZEK: We have three more questioners, Dan Bullen. But, you know, just thinking if you had more than two courses, you might have been really dangerous.

BULLEN: Bullen, Board.

I should probably preface my comments and questions by saying I'm a nuclear engineer, not a soil physicist or a geologist, and I've never had a geology course, so this is going to be even worse.

First off, maybe just a question of scale. When you mentioned proximal and distal for these alluvial fans, is there sort of a--how many kilometers, how many meters is proximal and distal? And, I know it depends on slope and all the other things that are associated with how these are developed. But, is there kind of a rule of thumb, you know, you're mostly proximal when you're within a kilometer or two of the mountain, and you're distal when you're five kilometers away?

MCDONALD: That's a good question. I mean, a good example is Death Valley. If you're on the east side of the basin, the alluvial fans are very steep and are very small, so it's the more tectonically active side. If you get on the west side of the basin, the fans are very long, almost like fan terraces. My rule of thumb, if I can walk along, and I'm not tripping over boulders, I'm probably distal. If it's a nice leisurely walk. If I'm climbing and I'm walking around boulders, I have to watch where I'm stepping, I'm probably proximal.

BULLEN: Okay. Can you go back to the scale where you showed the lake levels, and then the formation of the fans, just one of those--

MCDONALD: One of these ones down this way?

BULLEN: Yeah, one of those. What's the scale on the top two figures, for example, when you say you've got fan deposition?

MCDONALD: It's really relative, but it's really the larger of the size of the loop, the bigger the event. For instance, here, the Qf5, the Qf2, those are much larger fan depositional events as far as the size of the fans, the area they cover, and even the thickest of the sediments compared to the ones we see since then in the last 8,000 years.

BULLEN: Okay. And, then, along those lines, similarly with the top scale, is the time scale, I mean, it just happens to be deposited over the same time that the lake levels were in existence? And, I mean, I know how you can actually date the lake levels, but how do you date the time scale for the fan depositions?

MCDONALD: We have, in this case, this record is a variety of dates. Most of these are associated with radiocarbon dates, either on sediments, either within the sediments, feather of the fans are either buried by or cut through. In the case of, say, Soda Lake, the fan deposits are actually tied into wave cut platforms formed by the lake. So, there's a variety of geomorphic stratigraphic, and then we have other things like cosmogenic dating and other things, which are really more in the older fans.

In this case, also, the case of Providence, we've used the bracketing sand sheets, basically in some case the Qf5 is actually sandwiched between two different Eolian units. We use luminesce as dating on those sand sheets to bracket the period of deposition of sand sheets. We bracket the fans based on the periods when the sand sheets were being migrated and accumulating.

BULLEN: Okay. Bullen, Board, again. To follow up on that same kind of deposition question. Are these depositions that occur when the climate change, do they take long periods of time to deposit, hundreds of years, or do you get very large depositions with episodic events? Like, if I get a 500 year rainfall, for example, do I get just a potload of deposition, and then I may sit for another, you know, 20, 30, 50 years, and then have another big event? Or is it more steady state kind of deposition?

MCDONALD: I think those are really important questions. It's probably going to vary on the size of the drainage basin, and the type of material. I think both of those are going to occur. I think in some cases, you're clearly going to have very large--you're going to have a storm that, you know, if you want to call it your 500 year storm, 100 year storm, whatever it is, it's clearly going to move a lot of sediment. I think do I look at these in a journal sense? These are periods where we're basically transporting a lot of sediment from the basins out--from the drainage basins out to the alluvial fan environment. So, I see these happen in, you know, maybe a few thousand years, or a few tens of thousand years, the bigger fans. But, I think we're looking at just a mass movement of material from the basins out, and that could happen in big events, but it's probably just overall a greater degree of material being transported out.

BULLEN: Bullen, Board. Last question, I promise, Mr. Chairman.

Can you go to that last slide where you showed the weather patterns coming into northern California versus the southern? The average storm track at, you know, 25 to 10,000 years ago, you show coming in sort of from the south, southwest there. The question that I have for you is did the rise of the Sierra Nevadas during that time frame, and I don't know how much it rose in those 25,000 years, did that have an impact on the type of storm pattern and deposition that you'd expect to see?

MCDONALD: I don't think the Sierra, I think in the last 25, the impact would be too small. But in the older fan record, this is just food for thought, the older fan record in the Mojava, one question we've raised is you go back a million, two million years ago, how does the height of the Transverse Range impact alluvial fan record? I mean, those mountains really are coming up fast. If those mountains were lower, this goes for the test site, too, how would that impact the way the storms cut across the region if you have lower mountains. So, the last 25,000 years, may not have much impact, but if you go back a million or two or three million years, I'm curious what sort of impact that would have as far as the Transverse Range, for the same reason you're thinking.

BULLEN: Thank you very much. I always learn a lot.

PARIZEK: Priscilla Nelson. And, we can recruit these guys in the geology program.

NELSON: Nelson, Board. Unbelievable. Okay.

I would like to ask something a little bit different I think about the fans themselves as materials left. They're well known generally as places where water moves, water can move through fans in certain directions, certain locations that is used in many cases, like the Canaqs or over in Iran, Iraq, of moving water through. So, the sense of having water movement inside of a fan is a little bit different from what you've been talking about, which is depositional, and the stuff that happens at the surface. So, I'd like you to just think a little bit about that.

And, in particular, two things I think, one about what do your studies show about for these fans, how water moves through them, and, secondly, do you see evidence of post-depositional modification in terms of the class or increase or decrease in cement? What's happening post-depositionally to the texture of these materials, given that they're not pervasive laterally, because of the environment and deposition, but once deposited, what's happening?

MCDONALD: Well, I'm going to answer the last question first. If I understand your question correctly, what you're saying is we get these alluvial units, even soils, in buried environment, are they going to change?

NELSON: Nelson, Board. I think that--I expect that they will change over time. And, in this particular environment that you have, that are relatively near the site, what kinds of internal modifications that might actually change permeabilities and change flow?

MCDONALD: I can answer that two ways. One is this goes back to the question about buried soils. One of the greatest challenges in trying to identify buried soils is you want to know what's petrologic and what's geologic. And alluvial fan environments, and many other environments, once you bury that soil, or you bury that deposit, it will change, especially in the vadose zone, or even the saturated zone. You get a variety of silica or carbonated cements filling in the pores, you're driving cementation. You're clearly going to get some chemical changes.

One of the biggest challenges I have seen in buried soils in alluvial fan environments is that you can accumulate calcium carbonate so many different ways. And, one of the biggest challenges, how do you separate a groundwater carbonate from a soil carbonate? That's a real challenge. So, that's sort of a way of--I mean, we clearly know these things are changing as they're buried, and they'll come back to the flow path, I mean, certain alluvial units will control where the water is flowing and how it's flowing.

I see cases where a preserved buried soil at the top that will serve as a conduit with flow across the top of it, you'll actually see clay accumulation and silica cement forming above the soil. It looks like a buried soil, the top of a buried soil. The lower one is actually the buried soil. So, there are ranges or changes that will occur. Basically, it's almost like weathering or something. You're moving water and you're moving dissolved components. You are going to change this material.

PARIZEK: Thure?

CERLING: Cerling, Board.

I guess this is a good one to start on. One of the figures that you showed related to this was that you had about a doubling of rain in El Nino compared to non-El Nino sort of years. And, I was just wondering if your pluvial or your wet episodes, do you think those are related to El Nino or monsoonal driven rains, because one is winter versus summer?

MCDONALD: Clearly, I didn't go into this topic. Clearly, the monsoonal impact is huge in these alluvial fans, and how that relates when we've got--actually, in monsoonal type storms, you have the high intensity, which clearly can be really important for driving runoff and driving sudden depositional soil, so on and so forth. The frontal storms might be a big impact on vegetation that covers hill slopes, so on and so forth. I think one of the big questions right now, as I alluded so, was that we know we've got change in vegetation on these hill slopes, even the valley bottoms. We have different types of storm patterns, both monsoonal and frontal. How these come together to drive these regional periods of alluvial fan deposition, I think that's the next big question we've got to address.

I often run what I call the Bill Bull model, the Bill Bull who studied alluvial fans across the southwest for years, his idea was as you change climate, you change the vegetation. In other words, you go from wetter to drier, you change vegetation on the hill slopes, as you decrease vegetation and increase soil and stability, which drives sediment yield, which causes fan aggredation. So, you remove the plants, remove the soils from the basins, the side slopes and the drainage basins, and the transport those eroded soils out, and that drives the alluvial fan aggredation. That's sort of the classic model we run. I'm not sure if I believe that model in its entirety, but it does make us think about how do you take vegetation change, which climate change, different types of storm patterns, high density, high frequency--long and short duration, high and low intensity, how we pull this together to drive alluvial fan aggredation. You've got different parts. You've got sediment sort of in the slopes and the valley bottom, and you've got to move that sediment out in the basin and on the valley bottoms. How do you do that? It's a multiple step process.

So, I'm not sure I answered your question, but I think this linkage, we know climate change, some part of climate change has got to be driving these periods of fan deposition. But, exactly how that occurs, I think there's some big questions there.

CERLING: Okay, thank you. Cerling, Board.

What you showed was sort of three different things that happen on these fans. One is fans are deposited. Slightly after that, there's a period of Eolian deposition, but that doesn't necessary have to take place. And, then, there's another period where you didn't show anything. And, during that period, is that an erosion period? Is that a period where soils are predominantly developed, and then that would lead to the question that do the soils preserve preferentially the sort of those non-depositional or possibly erosional intervals?

MCDONALD: That's a good question. Let's see if I can answer that. There's probably more than one way to address that. Taking it from the top, clearly, the record I've shown, the record we have, we know that's a record of preservation. What we're seeing, we don't know if that's the entire record. That's the record of depositional events large enough to be preserved. The case of Eolian deposition, there's always dust and sand blowing across the desert, but we do see these discrete periods where there seems to be a pronounced increase in this activity, like with the fans.

I think--what was the rest of your question? I think this is always going to be a challenge, this environment, is that clearly, we have many periods--let me back up and say it this way. To my experience, I look at the desert environment, the geomorphic record. I'm often seeing what I think are periods or intervals of more discrete aggredation. So, we're seeing larger scale events, which I think helps in the preservation of those events. But, during the same time period, these events recur, and we clearly have fan deposits coming down the mountains. We clearly have sands blowing around. I think this is a matter of scale. So, the most simplistic interpretation of the record is we're preserving the largest events in the record, those ones we recognize. The smaller events in between, may or may not be preserved, and may not be recognized. I'm not sure that correctly answers your question.

CERLING: Then just as a matter of clarification, what intervals would the soils mainly be preserving? Because, clearly, actually aren't very tied to those large events.

MCDONALD: Right. Clearly, you have an active period of deposition going, aggredation, soil formation is not going to be preserved. Or, if you will, soils will be stretched out over the depositional interval. You have to have some degree of surface stability to form a well developed soil. So, if you have an active period of aggregation going on, you're not really getting much in the way of soils to preserve that, or they can be very--the soils will be difficult to recognize. So, if you look at it geomorphically, you could argue that the soils are forming between these events, but I would argue that I'd also add that soils are always forming. It's really a question of geomorphic stability.

CERLING: Yeah, that's fine.

PARIZEK: Consultants, questions? Staff?

If not, we thank you very much, Eric, for a good presentation of the fan story. And, we'll go to our next speaker right on schedule. That's Saxon Sharpe. Saxon is Assistant Research Professor in Paleocology at Desert Research Institute, and the research focuses on interaction between biotic systems and climate, how climate variation can affect individual species and communities, particularly molucks and plants, and how they respond to climate change.

So, we're very happy that Saxon could now give us some discussion about what the climate story is, and, again, the program takes basically three climate states, with some variations to it, and the idea there is a climate record that's been developed in the Great Basin area. We heard some consequences of it in terms of the fans (inaudible). Now, we'll see what the climate model shows.

Saxon?

SHARPE: Well, Eric's talk was a great segue into mine. In fact, I'd like to start out, if you can visualize that last slide with the two storm tracks, you had the Western United States, and during the glacials, the storm track was much lower, much more south. And, what is going on there is that you had a completely different circulation pattern of atmospheric circulation during the glacial periods, and I'll go into more detail on that. But, essentially, the jet stream was pushed much lower, and that was bringing those storm tracks in. So, that's a little bit of what I'm going to be talking about.

And, I wanted to mention to Dick that it was three years ago that I gave this talk, not just one. So, time flies.

PARIZEK: Then, there must be a lot of progress in the climate story.

SHARPE: Well, the last million year forecast is the same. Nobody has changed their vacation plans. It's okay.

So, anyway, today, I'd like to present the rationale for past climate being the key to future climate, and I'm going to really focus on that theme throughout the talk. And, I also want to present a long-term view of climate, so that will put the last 10,000 years and the next

10,000 years into perspective.

So, Yucca Mountain climate is driven by mechanisms operating on different spatial and temporal scales. They range from the largest and longest, such as the orbit and tilt of the earth and global atmospheric and oceanic circulation patterns, to smaller synoptic scale features such as ridges and troughs, the jet stream, fronts and high and low pressure centers. Small still are physiographic features, such as the location of the Sierra Nevada to the west of Yucca Mountain, which creates a range shadow there, and Yucca Mountain's latitude, which places it under the influence of the mid-latitude westerly winds and associated storm systems.

Finally, local topography creates variation in temperature, precipitation, and wind speed and direction. So, these processes have been operating and interacting for tens of thousands of years to create what we call climate.

So, I want to begin with three main points here for you to keep in mind as I go through this talk. The first is that past climate encompassed higher, sometimes much higher, effective moisture relative to today, and effective moisture is commonly defined as precipitation minus evaporation. And, greater effective moisture can mean increased precipitation or decreased temperature or both. So, it's not always increased precipitation for effective moisture. If you get low temperatures, you're also going to get more effective moisture.

Secondly, precipitation was often higher and/or temperature lower in the past because tropical moisture-laden air was coupled with colder air masses over the Yucca Mountain area. So, that's like that jet stream that I talked about dropping south.

Third, infiltration was commonly higher relative to today because water is stored more readily during periods of greater effective moisture.

I want to begin with four assumptions that we need to have to use past climate to estimate future climate.

The first is that climate is cyclical. The past is the key to the future.

Second, that a relation exists between the timing of long-term climate change and orbital parameters. And, I'll be discussing these more, these first two, when I talk about the Devil's Hole record coming up.

Third, a relation exists between the characteristics of past climates and the sequences of those climates. Essentially, you have kind of segments of 400,000 year climate episodes, and there are generally four glacial periods within each one of those episodes, and the sequencing, the magnitude and the sequencing of those glacial periods seems to be consistent for the last 800,000, 400,000 year period, and the 400,000 present day period, and we're going to go from present day to 400,000 in the future period with that same sequencing.

And, then, finally, that the long-term earth-based climate forcing functions have remained relatively unchanged for the last 500,000 years, and should remain relatively unchanged for the next several hundred thousand. I won't have much time to talk about that, but that's essentially like tectonic change, like someone brought up, the rising of the Sierra Nevada, creating a range shadow effect.

These are the four steps that we use to forecast future climate, and I'll be going through each one of these in order, and I'll spend most of the time on the first one, because that's the main point right here. And, I want to give credit to Rick Forester of USGS who developed this methodology in his AMR in 2001. The material that I'm presenting here essentially takes the same methodology that he came up with for the next 10,000 years, and takes that methodology into the future to estimate future climate change up to 500,000, or even a million years in the future. And, the timing that I came up with corroborates his results. So, my work essentially just extends that time period.

So, first, I want to compare the relation of the Devil's Hole record to calculated orbital parameters to identify past climate pattern. Then, I'll talk about projecting this pattern into the future to establish the timing of future climate regimes, because essentially, the orbital parameters can be calculated for both the past and the future.

Third, identify the magnitude and nature of past climate states, and we simplify these to just four climate states, essentially Interglacial, which is the modern climate state, Intermediate climate state, Monsoon climate state, and Glacial climate state.

And, then, finally, present-day meteorological stations were selected to represent those past climate states.

So, first is to compare the Devil's Hole record to orbital parameters. And, Devil's Hole is located about 60 kilometers south, and a little bit east of Yucca Mountain, and it's an accurately dated calcite vein that records the isotopic variation in atmospheric precipitation in the recharge area from the regional aquifer from about 568,000 to 60,000 years before present. The Devil's Hole record compares well with other regional and global climate change records. So, it appears to be an excellent chronology of global climate change in the lower troposphere. And, the Devil's Hole record is extremely well dated.

This is Slide 7 in your handout. I know it's difficult to see on this screen. But, these are different proxy records for glacial and interglacial climate. This is present day climate right down here. Time is along the bottom axis. This is 800,000 years ago. The first six are proxy climate records from the Southern Nevada, Southern California area, and the last two, this is a lake record from Siberia. These are lake sediments. And, this is an ice record from Antarctica. And, you can see that they compare fairly well with each other. There are long periods of glacial and interglacial climate. Oh, I should say that the upper, I think the upper is glacial and the lower is glacial, but essentially, they are generally synchronous over time. There is a little bit of discrepancy in the timing of them, but that's par for the course with different proxy records.

Essentially, this is saying that the Devil's Hole record does seem to be a very good record of regional and possibly global climates.

This is comparing the Devil's Hole record to orbital parameters, and I'll spend a little bit of time on this. On the X axis, this is time, 500,000 years ago, to 250,000 years ago. The next slide takes you 250,000 to present day. The Devil's Hole curve is in red here. The peaks are interglacial periods, and the troughs are glacial periods. And, that's the oxygen isotope. Those are the oxygen isotope values for Devil's Hole on this axis. This axis graphs both of the orbital parameters, and these are the ones that can be calculated, both past and future, because they're calculated through the gravitational pull of other bodies, other planets on the earth.

The blue line is the eccentricity, that's essentially the orbit of the earth, whether it's more circular or less circular. More circular are these minima down here. The precession index is the black line, and that's a variation of seasonality, or results in a variation of seasonality within the earth. The peaks for precession up here are southern hemisphere summer radiation maxima, which this corresponding dip down here where there's nothing would be, of course, the northern hemisphere, southern radiation maxima. So, these points, you've got southern hemisphere, down here northern hemisphere radiation maxima.

The colored blocks are interglacial is red, glacial is blue, and intermediate climate moving from either interglacial to glacial or glacial to interglacial is the transition climate. Now, these colored blocks are based totally on the precession. They're not based on the record of Devil's Hole. So, this is showing that there is a correspondence between the Devil's Hole interglacials and how you can use the orbital parameters to estimate both past and future climate.

And, I should say here that often workers define an interglacial period as about the middle of this transition from glacial to interglacial periods. So, from about in here to where it drops down to about the middle in here, I am defining the interglacial periods for the purposes of this study as the high peaks right in this area. And, that way, you get more climate states, because certainly, say, this climate, whatever this climate is right here, moving from glacial to interglacial is a different climate state than what you have up here, or what you have moving from interglacial to glacial.

So, basically, how this work is you take the eccentricity minima, so you've got three of the minima in this graph, that's marked as an M, with the solid vertical line. To find the termination of the glacial, you move from the minima point down to the very first northern hemisphere, southern radiation maxima. And, that is essentially the termination of the glacial period, as you move from a glacial period toward an interglacial period. Now, there are a series of reversals on both sides of the interglacial, but essentially this is where things begin to change, and get warmer. To determine this I event, which is the end of the interglacial moving toward a glacial period, you go from the T point, hop over to southern hemisphere, summer radiation maxima, and that is the termination of the interglacial period. And, when I get to the next slide, you will see that we are right at an I event right now, so we, according to this methodology, we're at the end of an interglacial, moving into intermediate climate state, moving toward a glacial.

This is the next slide, where we have 250,000 years ago, and present day, essentially all the colors and things are the same. Oh, I wanted to just point out at about these 400,000 year cycles right here where we have an eccentricity minima, we also have precession, very low amplitude, and that's why a number of people think that this time period and the time period we're beginning to move into, you know, next 400,000 year cycle, are going to be similar, because the eccentricity modulates precession.

You can see that the amplitude of the precession parameters from 250,000 to present day are much higher. Essentially, everything is the same, colors and everything, as the last graph. The frequency of the precession cycle also denotes how long the different climate states are. So, as you get these higher amplitude precession cycles, the climate states tend to get a little bit longer.

So, now that we've got kind of a match between the Devil's Hole record and the orbital parameters, we want to project this pattern into the future to establish the timing of future climate change.

So, here is the future graph, zero, present day climate, 250,000 years into the future, 500,000 years into the future. And, again, here we have an eccentricity minima, with a very low precession amplitude right here, and at 400,000, again, there's a minima and this low amplitude. So, you can see part of that 400,000 year record, and that's shown in different climate proxy records throughout the world where you have evidence of similar climates happening every 400,000 years.

I want to go back actually. I forgot to mention these isotope stages, MIS7 and MIS5, MIS3, that stands for marine isotope stage, and the odd numbers are interglacial periods, the even numbers are glacial periods, and these are also found in climate proxy records worldwide. They were designated probably in the Sixties, and they're not synchronous across everywhere, but essentially, the glacial and interglacial states are often referred to as MIS stages. And, in terms of the sequencing, when I was talking a little bit about the 400,000 year records where we have an MIS6, this in a number of terrestrial and oceanic records, the marine isotope stage 6 is a very cold, wet, glacial period relative to the other glacials. MIS4 and MIS2 were cooler and dryer, compared to MIS6.

If you go back to MIS8 and MIS10, which are older, those were warmer and wetter compared to these two states. So, essentially, the 400,000 year sequence goes kind of a warm, wet interglacial, which would be equivalent to 10, another warm, wet, a very cold, wet, and then a cool, dry glacial.

So, into the future, this is what we have estimated. These are the equivalent of--this is the equivalent of a marine isotope stage 10, which is a warm, wet, isotope stage 8, another warm, wet, and then cool, wet glacial here, equivalent to a 6, and then a cool, dry glacial, which is equivalent to an MIS4, or actually, MIS2.

The glacial states for the future, there are five of them here for the next 400,000, 500,000 years, and they will vary in length from about 8,000 years to 38,000 years, and they will have different magnitudes. And, the glacial states are certainly the ones where there is going to be more infiltration. These intermediate climate states are still cooler and wetter than today, but they're not as cold and wet as the glacial states.

Just as a little test of the precession methodology, I wanted to compare the length of the glacial and interglacial states with the Owens Lake record, which is this pie diagram right here. This is based on lake proxy data, totally different from Devil's Hole, so this is a different climate proxy record. And, then, these two pie diagrams, this is the last 4,000 years based solely on the precession methodology, where those glacials or interglacials begin, and then that's past and this is future. And, there's less than a 10 per cent difference between these three, which

I think is a pretty good match. The glacials match pretty well, 21 per cent for Owens Lake, 23, and 19 per cent.

The interglacial Owens Lake is quite a bit longer, 20 per cent. This is 13, and I think that's 13. The Owens Lake record, there's a little bit of problem with the dating. It's not a continually dated record, so the dates are interpolated, so there's probably some slope between climate states there. But, this compares fairly well.

Okay. So, once the pattern has been projected into the future, we want to identify the magnitude and nature of past climate states. So, these are the four that we came up with. The modern climate state, or interglacial, intermediate climate state, monsoon, and glacial climate state.

Okay, Owens Lake, California is about 160 kilometers west of Yucca Mountain. It's a present day playa, which contains a thick sequence of lacustrine deposits. The core spans about 850,000 years, and it records snow pack in the Sierra Nevada. And, essentially, this is the first long record that we've taken for comparison, because we get a really good idea of the magnitude in the Owens Lake record. There were a number of different studies done on this core, and the magnitude for this study was based primarily on the ostracod and diatom record in the lakes, but it was also corroborated by geochemical data and other studies that were done on the core.

Death Valley, California is also another record. Death Valley is about 100 kilometers west of Yucca Mountain, and it has a 200,000 year lake record, and Death Valley contained deep and fresh water and saline lakes that were supported by the Amargosa River flow and tributaries such as Fortymile Wash. The lake in Death Valley was 175 to over 300 meters deep, sometime between 180,000 and 120,000 years ago.

Local records also helped us determine the different magnitude climate states. Springs and wet winds were common on the valley floors during the different glacial periods, and packrat middens, we collected a number of them and got a pretty good record of vegetation growing during the glacial periods. Both the spring and wetlands and packrat middens, we estimated the last glacial, which was marine isotope stage 2, centered about 18,000 years ago. The mean annual temperature was about 8 degree celsius, and mean annual precipitation was about 300 millimeters per years.

So, the next thing we needed to do is come up with the magnitude of climate states, and what that sequencing, so the very simplified climate state sequence was this one, interglacial and glacial periods with transition periods in between. The monsoon climate stayed essentially--that's a pulse of monsoonal circulation coming up from the Gulf of Mexico, or off of the Pacific, so you just have these short, maybe 300 to 1,000 year pulses where you get the monsoon, but we had to simplify it for input into infiltration models. So, this, we feel that these four climate states capture the variability of past climate and future climate.

In terms of the different magnitude of climate states, I've talked a little bit about how we used the last glacial period to estimate, to come up with kind of a calibration with the material that we collected, and these are the relative states, with increasing temperature here, increasing precipitation here, with interglacial climate, and then the glacial climates over here. these are the three magnitude climate states that I talked about for the sequencing, with the intermediate climate state in between.

In terms of the characteristics of these climate states, the modern climate is hot, very dry summers, with convective summer thunderstorms associated with a thermal low over Southern Nevada. There's monsoonal activity when Southern Nevada is under the influence of the sub-tropical highs. In the intermediate climate state, we had warm to cool and dry summers, with cool, wet winter season and winter dominated precipitation, with greater effective moisture. Essentially, these different climate states are occurring because you have the high and low cyclones and anti-cyclones moving around over time.

The monsoon system is warmer and wetter than today, and the monsoon period had increased summer rainfall, with most of the annual precipitation falling in the summer. Glacial states, again, different magnitudes, all have much greater effective moisture than today, with increased precipitation and/or decreased temperature. The winters were cold and wet, or cold and dry, and the summers were cool and dry, or cool and wet.

Note that the modern climate state has lower annual precipitation and higher annual temperature than all the other climate states except the monsoon.

So, finally, we needed to select present day meteorological stations to represent those past climate states, and by selecting those stations, there were values, both daily and seasonal values, that were available for input into infiltration models.

Again, here's the similar graph as the last one. But, instead of the bubbles, we have actual numbers here. Increasing mean annual temperature here, increasing mean annual precipitation here. These are where the different climate states fall in temperature and precipitation space, if you will. The modern climate at Yucca Mountain is right here, and these values were determined using Nevada Regional Stations 3 and 4, which is essentially the southern part of the State of Nevada.

The monsoon climate state up here was determined by

Nogales, Arizona and Hobbs, New Mexico because we felt that that represented the monsoonal flow coming up from the tropical Pacific, or possibly from the Gulf of California.

Intermediate climate state, and these all have upper and lower bounds, and we felt that that would capture the variability within the different climate states, so the intermediate lower bound that was Delta, Utah and Beowawe, Nevada. The upper bound for the intermediate climate state is the same as the glacial lower bound. So, this is the warm, wet glacial period, and that was represented by the stations of Rosalia, St. John and Spokane, Washington. Upper bound for this period was just north of this, Chewelah, Washington. As we move into the cooler and wetter glacial climate states, this lower bound is Elko, Nevada, the upper bound is Browning and Simpson, Montana. And, then, this is the very cold, wet glacial, with the upper bound is Lake Yellowstone, Wyoming.

And, these stations were chosen essentially because if you remember Eric's graph with the circulation being pushed, or the jet stream being pushed much lower to bring wetter climate into Southern Nevada, because the sub-tropical high that we have off the coast here during modern climate states was not as prevalent, it wasn't as strong, it probably moved out into the Pacific, which allowed the Aleutian low to move down closer, making jet stream circulation come right through the southern part of Nevada.

So, in past climates, we had a very, very different circulation pattern set up, so that's why these sites were chosen throughout the western United States, to try and capture where the jet stream is today. So, essentially, in the summer, it resides up here, which is why the stations were more northerly than what you might think might represent climate in the past if you brought the stations down in here.

So, in conclusion, the modern climate state is estimated to last about 600 more years. The monsoon climate state is estimated to occur from about 6,000 to 2,000 years after present. Intermediate climate state, about 2,000 to 30,000 years after present. And, the glacial climate state, 30,000 to 50,000 years after present. And, just remember that modern climate has less effective moisture and the total modern climate is of much shorter duration than either the glacial or the interglacial climate states.

Continuing on, the past and future climate may be represented using four major climate states. Again, there were many more, but they can be broken down into these four, with upper and lower bounds. There's a close match between the Devil's Hole and calculated orbital parameters, and that provides the rationale for past climate being the key to future climate. And, the nature of future climate is based both on the nature of past climate and the assumption of cyclicity. The nature of future climate is based on the sequencing and characteristics of past climate.

That's it.

PARIZEK: Thank you very much. It's a lot of material. Some of the graphs our plots don't show. I think on Page 11, we have gray boxes, Page 9, Page 8, whereas you have data that goes in those box areas. I don't know whether we might be provided a copy. You have a lot of detail in there that would be helpful for us to understand.

Now, I think you must have given a talk within the year that I heard at GSA?

SHARPE: Yes.

PARIZEK: That's good, because then error bars are being reduced from three years to one. I feel better about that.

Questions from Dan Bullen?

BULLEN: Bullen, Board.

Actually, if you could go to your first conclusion slide? As you try to make predictions of modern climate 600 years from now, could you comment a little bit about the effects of global warming, I mean, the man made or human made effects of what that might do to climate? And, sort of the relative magnitude of that, versus the types of magnitude you'd expect with respect to the orbital changes?

SHARPE: Okay, let me go to this slide. For potential global climate warming scenario, the temperature estimates are much better constrained in precipitation. Precipitation is basically all over the place for the western United States, but in terms of both the Intergovernmental Panel on Climate Change, and another study that was done that had a little bit higher resolution, this was by USGS, Thompson, et al., I think about 1999, they're indicating warming, both warming in the summer and in the winter, and Thompson, et al., the IPCC does not have specific values on how much warmer it will be in terms of temperature. Thompson does, it's two to three degrees in the winter, and three to four degrees in the summer.

So, if you look at the monsoon climate state, that would encompass the temperature part of global warming, if those studies are correct, because this is about 13 degrees here, and this is 17 up here. So, that would encompass it.

Now, as far as precipitation goes, the jury is out on that one. It may be more, it may be less. If it's more, it certainly isn't going to be way up here, at 400 millimeters, you know, they're guessing maybe a 10 per cent increase I think maximum. And, Thompson's study suggests that there's going to be a decrease. So, that would be putting it down here somewhere. So, with, of course, with less precip., there would be less infiltration. So, I feel that, you know, this trajectory captures at least the studies so far with climate change.

BULLEN: Bullen, Board. Just a follow-on question.

What's the expected duration of the global warming effect, ballpark? I mean, I know there's a lot of estimates.

SHARPE: Eventually, we're going to run out of fossil fuels. There are a number of different estimates on that. I've read someplaces where it may be 10,000 years into the future. I mean, say, we run out in 300 or 500 years, and CO2 begins to drop off, we don't know what that mechanism is going to be, how that's going to be sequestered. So, it could end up going out 10,000 years into the future in terms of the perturbation that we may be causing right now.

BULLEN: Bullen, Board.

That's actually a very important parameter, because of the fact that the thermal pulse of the repository only happens at about 1,500 to 2,000 years. So, whether or not it's wetter at the repository horizon during that time frame is kind of important.

But, the last question I have is with respect to the magnitude. Is the magnitude of the global warming effect going to be similar to or completely overridden by the orbital changes?

SHARPE: That's a really good question. I don't have an answer to that. I have no idea. We'll have to see.

BULLEN: Thank you. I don't expect to be around long enough to make those measurements, but thank you very much.

PARIZEK: Priscilla Nelson?

NELSON: Nelson, Board.

I'm sort of thinking about local climates, and micro climates. I realize this is a very large scale climate study that you're talking about, but I'm wondering about the variability within, spatial variability that's likely to happen, or could possibly happen within what you might call a climate state because of local effects. And, I note that you've got a variety of different kinds of proxy records that are being merged to this consideration that you're presenting here. Are there any proxy records obtainable in the Amargosa Valley that could be used to look at what's been happening there? And, you reported the Las Vegas Valley marsh deposits, which are out there sort of at the end of the fans, that area. There certainly are some features in the Amargosa Valley that could maybe be proxy. What do you think about that?

SHARPE: Yes, those studies have been done, or a number of studies have been done in Amargosa, primarily sediments, both looking at alluvial--or looking at sediments in washes, doing some coring in the playas, and those only go back to about the last glacial. So, you know, we're getting the last 15, 18, maybe 20,000 years within those sediments, and what that has shown is that the Amargosa did flow during very wet and/or cold periods. So, there is that proxy.

Again, the packrat middens, those are discontinuous records, but you can go in and get a midden, look at what vegetation is there, and determine what vegetation was growing in the past, and get some kind of parameters on past temperature and past precipitation. You know, your question would have to be answered by looking at discontinuous records, but there are records there, but they're spotty.

NELSON: Nelson, Board.

What do they indicate overall? That this kind of regional climate change is tracked for the Amargosa Valley, or do they indicate that it's at one end of the--

SHARPE: No, it's regional. Everything I presented here is regional, and affects the Yucca Mountain area. You know, essentially, it is under these controls.

NELSON: So, whatever proxies there are in the Amargosa Valley agree with this prediction?

SHARPE: Yes.

PARIZEK: Ron?

LATANISION: Latanision, Board.

Devil's Hole seems to be a remarkably prominent part of the, let's say, confidence building in the evaluation of the climate changes that are anticipated. Is it unique, or are there other equivalent sites on the planet, or is Devil's Hole a unique location?

SHARPE: Devil's Hole is really unique, and we are really lucky to have it right here as close as it is. It's essentially the only well-dated terrestrial record that we have. The dates are iron clad. There's no interpolation. I think every point, if you picture back the red dots on the Devil's Hole diagram, each one of those encompasses about 1,800 years, which is incredibly, you know, very, very good, and it does correlate with other worldwide records. The ice cores and ocean core sediments, very few dates, they've been interpolated, or they've been tuned to orbital parameters, like the spec map data, which is a series of stacked ocean core sediments were based on the obliquity parameter, which is every 41,000 years, and it was tuned to that. And, for a while, people were saying, well, Devil's Hole doesn't really correspond with that. But, they made that up. If they had tuned it to precession, they might have corresponded really well. So, we're really lucky Devil's Hole is a great record, and unique.

LATANISION: Latanision, Board.

Just out of curiosity, when was it appreciated? When was it identified and then appreciated for what it was telling us?

SHARPE: I think it was the mid Eighties is when I published that, I think mid to late Eighties.

SCHWARTZ: Schwartz.

Are there any controversies existing in the community regarding the relationship between glacial mechanics and orbital mechanics, or has that gone away?

SHARPE: There's plenty of controversy that exists. I mean, if you look at what we've done here with just matching, looking at the Devil's Hole record and the orbital parameters, that hasn't been done. Most of the glacial material--well, when you look at glacials, or moving into glacials, that's done by modeling, and essentially, the models can't really create a glacial period. We don't have quite the correct parameters in there. Maybe I'm off on a tangent from your question.

SCHWARTZ: But, I guess I was wondering how much uncertainty is there? I mean, you have a theory with respect to how orbital mechanics might produce some future glacial sequence, what uncertainty might be attached to that prediction, because you may not understand exactly how things work, or there's alternative theories out there that we haven't heard about this morning.

SHARPE: Right. Okay, I was kind of on track, but a little bit right. There are alternative theories. You know, one is the modeling, where a number of models suggest that we are going to be going into a long-term interglacial state where we have an interglacial climate for the next 50,000 years. That's based on I think double CO_2 in the atmosphere, and it's based on a model--I mean, I would bet on this, you know, if I had to stand up here, I would say looking at the past climate, because the model, you can't really verify it, the model doesn't really create climate, as we have seen it in the past, so there's a lot of uncertainty, a lot of controversy in terms of who you talk to about future climate. But, I would be willing, and I am betting that the past is the key to the future.

PARIZEK: Other questions from Staff?

One question about ice core record. This is Parizek, Board. It shows rapid effects, and you'd think maybe a land-based record would probably be more subdued, or take longer to respond.

SHARPE: Yes, in terms of the Devil's Hole record, again, you know, each point is integrated, and that's essentially tracking the regional hydrology. So, you have precipitation coming in and moving through the aquifers. So, that's getting damped a little bit, and there is a time lag there.

PARIZEK: Would that time lag be helpful in sort of model validation in terms of flow? I mean, is that just asking for too much?

SHARPE: Yes, I'm trying to remember, I'm thinking it was maybe like 2,000 to 5,000 year time lag, and I might be making that up, but I'm thinking it's not that long.

PARIZEK: I know one of the questions about the plot points for Devil's Hole, do we have Devil's Hole from 60,000 years to the present?

SHARPE: Yes, that will be published at some point in the future. Ike has that material, and that information, and it's going to be really interesting to see if the Devil's Hole record actually does what I think it should do.

PARIZEK: That would be sort of validation of other views. Sally Devil asked what if holes reverse? Would that make any difference to climate?

SHARPE: I don't know.

PARIZEK: Leon Reiter?

REITER: Leon Reiter, Staff.

Saxon, a number of years ago, the NRC sent to the Nuclear Waste, an analysis, did an expert elicitation on future climate. I wonder if you've had a chance to look at that, and how consistent is that with what you're coming up with?

SHARPE: Was that done about maybe six years ago?

REITER: Yes, something like that. I'm not quite sure.

SHARPE: Is that the one I'm thinking of? Yes, I have looked at that, and I think this is a much better way to go.

REITER: Are the conclusions different?

SHARPE: I think, and, you know, that was before I came into the project, so I'm not exactly sure what happened, but I think that that prompted a reevaluation of looking at past climate, and we went into much more detail, and came up with this methodology. Essentially, you know, this is more fine tuned, and it--well, it's more fine tuned and more specific than the expert validation effect.

PARIZEK: Any other questions from Staff?

Thank you very much. I feel better, and I think in the 30 day weather forecast, predictions you make are sort of constrained in so many different ways, so thank you very much. We had a great talk.

We have now time for a break. We are supposed to have a break until 9:55. I mean, we start at 9:55. So, we're a little bit ahead of schedule. So, why don't we come back at 10 o'clock, just to stay on track.

(Whereupon, a brief recess was taken.)

PARIZEK: Our next presentation, we'll look at climate change in Yucca Mountain unsaturated zone hydrology from the mineralogical point of view, minerals that are in the mountain. It will be presented by James Paces, who is a research geologist in the Yucca Mountain Project Branch of the U.S. Geological Survey, and is a member of the Environmental Science Team for the last 12 years, has worked on isotopes, geochronology and geochemical studies on surface deposits, groundwater, whole rock, fractured minerals and dust. Jim?

PACES: Thanks, Dick.

I didn't get the name, the title of this topic, and for those who want to know everything about unsaturated zone hydrology, might be disappointed, but as Dick said, I'm going to take the--one of the things that we've done in the last ten years, or so, is taken a look at the secondary minerals in fractures, lithophysal cavities, and I'd like to use some of that information to make a connection between what we see at the surface, what Saxon and Eric both gave us a very nice introduce to climate variability at the mountain, or at least in the region, and see what we can say from that perspective for flow through the unsaturated zone.

So, there's two scales of climate variation that we can look at in the past. First of all, we can look at the transition between Tertiary to Quaternary climates, and it's perceived that the Holocene and Pleistocene climate conditions were both wetter and milder, whereas Quaternary conditions were drier and more seasonal, that is, hotter summers, colder winters, and this transition took place around 2 to approximately 4 million years ago.

On a more recent time scale, we can also look at variations in Quaternary climate, which is what we heard about this morning. These are 100,000 year cycles that are related to glaciation in the northern hemisphere. And, in Southern Nevada, these cycles consist of generally colder and wetter pluvial periods, intermediate and monsoonal periods, and then warmer, drier interpluvials.

As Saxon told us, we can go ahead and extend to future climates by looking at the past. And, he and Rick Forester and other people have done this, so over the next 500,000 years, based on the analysis of orbital parameters and analog sites, we can expect there to be something like six glacial cycles, and the conditions in those, we expect are going to be similar to previous cycles.

We've made estimates of how much time we'll spend in each one of these different climate states. There's been estimates of temperature and precipitation, and that has been fed into an infiltration model so that there's estimates of what we should expect in terms of future infiltration.

So, what we want to do is take a look at some various different records of climate change. We have various different surface records, which give us something about the temperature and precipitation that occurred in the past through the studies of paleolimnology lakes, either chemical, sedimentological or peleontological evidence. We can look at paleobotalical evidence, packrat middens and pollen in particular, and as Eric told us this morning, sedimentology plays an important role. We can look at weathering, calcrete formation, eolian and pluvial processes.

We also have various different saturated zone records, and these can tell us something about the water tables, past fluctuations in water tablets, paleohydrographs. We know something about discharge deposits throughout the region in general, and in the Amargosa Valley in particular. There's also a very nice record at Brown's Room, which is a cavity in Ash Meadows, and tells something about past water table fluctuations. It also is important for telling us something about paleorecharge compositions, and I'm thinking in particular here of the marvelous record at Devil's Hole that Ike Winograd and colleagues have described, which tells us something about variations in the meteoric water composition.

We're a little less fortunate in the unsaturated zone, although we have a very thick unsaturated zone. It's difficult to look at. We've extracted some pore water at Yucca Mountain where we can look at oxygen and hydrogen isotope records. There's also some chlorine-36 work that's been done, which suggests that at least one model has it that there is higher values, chlorine-36 values, chlorine-36 to chloride ratios in the past related to geomagnetic variations. And, then, we've got secondary hydrogenic minerals in fractures and cavities, which is going to be what I'm going to talk about for the rest of the time period.

These hydrogenic minerals are important because they represent a long, probably more than 10 million year record, of deposition from water that percolates through the unsaturated zone. And, there's two types of information that we can glean, at least two types of information, related to climate change, and one of these is the growth rates of these minerals. Growth is controlled by both liquid and gas fluxes, and these can respond to climate-induced variations in infiltration and surface precipitation and temperature.

Also, the compositions, both isotopical and chemical, can tell us something about climate-related changes in the compositions of the recharging water at the surface, and of the conditions at the time of deposition.

So, just a quick slide. I think you've probably seen some of these materials before, either through some of these types of pictures, or actually underground. The secondary mineral coatings are distributed sporadically throughout the unsaturated zone. It's very nicely exposed within the tunnels. They're generally on fracture footwalls and cavity floors. The coatings are dominantly calcite, with less abundant silica phases, and these vary substantially between nice, thick centimeter scale deposits on low angle surfaces to think, more uniform thickness coatings on steep fracture. The textures themselves vary quite a bit from very complicated, bladed textures to more massive structures with internal stratification. And, then, a couple of slides just to show the complexity that we have to work with.

As with any record that's related to past climate, we need a reliable geochronological framework. And, fortunately, these minerals can be dated by natural radioactive decay. In particular, we're lucky that opal has a substantial amount of uranium incorporated into it. We can use this for several different dating schemes. Uranium series through 234, and uranium 238 model ages, and then lead uranium data dating. They all have different ranges, which they correspond to, and because they have large concentration, it lets us get away with a fairly small amount of material.

Calcite, on the other hand, does not incorporate much uranium, so we're compromised in terms of our U series capabilities, in terms of we need much larger samples to get a measurable signal. We do have carbon as a structural element, though, so we can look at radiocarbon. Unfortunately, we're limited to time scales in the last 50,000 years.

So, maybe a decade ago, or so, we started looking at outermost surfaces, thinking that these would be the most pertinent to the recent past. And, we were surprised, because we started to see Pleistocene, radiocarbon and U series ages for most of these deposits. We sort of expected that we'd be hunting for a few needles in the Yucca Mountain haystack, but in fact we started to see Pleistocene ages all over the place.

There were some problematic aspects with these early date, though. There was a wide range of ages for samples from the same outer surface in this series of histogram. It is that changing scale, zero to 50, zero to 500, and zero to 2,000 years in the past for radiocarbon, 234 uranium, U series dates, and then lead uranium ages. And, you can see that the loads are quite different for these different systems. We also tended to see the youngest ages, from the thinnest subsamples that we were working with, and that the isotopic systems with larger half-lives yielded older ages. I'm not going to get into the details of some of the uranium series disequilibrium studies, but we also say unexpected behavior that took us a little while to figure out what might be going on.

These problematic aspects forced us to sort of reexamine basic conceptual models about mineral deposition, and sort of 3-N member models here could be viewed as instantaneous, episodic or continuous. And, in the case of--this cartoon is just sort of thrown up here to give you a general idea of what we're talking about. And, in the instantaneous deposition, the entire coating is deposited at a point in time. It's homogeneous in composition initially. It evolves as a closed system, and it follows the fundamental radioactive decay laws, so that our little subsample, this block of mineral that we're cutting out of there and analyzing, should give us a calculated age that's very close to the true age of the material.

But, when we start to have thinner layers involved here, each layer may have been deposited instantaneously, but now our subsample includes a number of different layers, each of which may behave as a closed system, and may have been initially homogeneous. But, our sample now includes all of this different material, and there's no way a priori for us to figure out which atom came from which layer, so we've got some kind of averaging going on, and that can be taken to the extreme if our deposition is continuous and layers are small, we can start thinking about this in terms of an integral age, where our subsample may really give us something quite different than what we expect. This effect is particularly substantial when the growth rates approach the rates of radioactive decay of the systems that we're talking about.

So, by adopting this numerical model of continuous deposition, we were able to predict a number of features that gave us heart burn before. We get positive correlations between age and subsample thickness, so that the thicker the sample, the older the age. This is sort of the observed range here. We also predicted, although we didn't measure growth rates directly back in those days, we predicted that they should be slower than about 5 millimeters per million years, and it also gave us a very elegant way to account for the discordance between ages of different isotopic systems.

This is our conventional or calculated age, our age calculated in the conventional manner versus true average age. One to one line would mean that we're doing a very good job of reproducing conventional and true, but you can see for these different short lived half-life systems, that's radium 226, carbon 14, protactinium, uranium series, and then uranium lead. They all seem to plateau out at younger than true ages, this particular model was run with zero age material on the outermost surface.

Also, we saw uranium series systematics that tended to mimic the patterns we observed. And our conclusion then was that the measured isotopic compositions are mixtures of younger and older materials, for the most part, and that thinner is better, the thinner samples yield calculated ages that should be closest to the true average ages that we're looking at.

We also then moved from just working with outermost mineral surfaces. We became curious as to what the integrated history of deposition was, so we moved in the direction of uranium lead dating. We're in two year layers. Basically, these uranium lead dates are typically concordant with the microstratigraphy that we see. We're looking about 3 centimeters worth of material, the base of which is about 7 million years. The green here is an ultraviolet light, photograph, so green represents uranium rich opal. The blue represents uranium pore calcite. And, we see around 4 million year old opal in the center of this, and then around 100,000 years for the outer surface in this particular case.

You can also see that we've got a wide range in ages for these various different materials, dating back to around 10 million years. We haven't been terribly successful at filling this gap. But, at any rate, we can use these histories to calculate long-term average growth rates, and when we work out the depth/age relationships, we see the average Tertiary growth rates are typically between about 1 and 5 millimeters per million years.

These growth rates are maybe thousands to more than millions of times slower than published speleothem growth rates, but they are generally consistent, no matter where we look within a coating, those average growth rates seem to be fairly consistent, suggesting that there is a more or less uniform long-term average growth rate in play.

At the same time that we're trying to date these, we're also looking at other isotopic compositions in the mineral coatings, and in particular, we've looked at oxygen, carbon and strontium isotopic compositions. We see that they vary with microstratigraphy. In the crudest sense, we can sort of break these out, categorize them into an early and intermediate and a late stage depositional structure, and then by applying uranium lead ages to interpolate, opal and chalcedony, we can start working out a framework, some typical values for these different systems. I've also included here for the early and the late. We can move on.

I think that carbon has been particularly informative in terms of climate variations. The histograms on the left-hand side of the plot show that there's a general evolution of compositions with plenty of overlap, but nevertheless, early stage is generally greater than around 2 per ml. of Delta C13. The intermediate stage has the dominant mode, between about -4 and +2, and then late stage is dominated by a nice mode between about -8 and -5.

We have interpreted these changes to reflect different signals from incoming meteoric water. Tertiary conditions which were wetter and milder, supported dominant floor of grasses, most likely. They have a photosynthetic pathway, it's been termed C4 type photosynthetic pathway, which ends up, the important thing is that it ends up with the soil calcite that has a Delta 13C composition of around +2 to -5 per ml. Whereas, during the quaternary, with a drier, more seasonal climate, we started to incorporate more shrubs and desert succulents. We're looking at a mixed C3, C4 photosynthetic pathway for the plant community at the surface, giving us a more negative value, -5 to -8.

When we apply our dating and compositional information together, we see that this transition occurs probably somewhere around 2 to 4 million years ago, and it corresponds with a major shift that we see throughout the northern hemisphere with the onset of glacial conditions in the quaternary.

If we look at compositions on a more recent time scale, we can use Devil's Hole record that Winograd and co-workers have developed. It's sort of a yardstick by which we compare everything in this part of the world. So, over the past 600,000 years, oxygen has varied cyclically between about 13 to 16 per ml. And, this reflects a change in the mean annual temperature, with higher values being warmer, lower values reflecting colder conditions. Saxon showed this in a much more expanded version earlier this morning.

But, carbon also shows a similar record. This time, between about -3 to -1.5, and it's perceived that this also reflects some kind of change in vegetation. But, as you can see with the two plots on top of each other, there is definitely a very strong negative correlation between the two signals.

If we look at this kind of information in our unsaturated zone calcites, we see that they have similar total range of variation, about 3 per ml. for both oxygen and carbon. What we're looking at here is the entire 10 million record, but I've got highlighted in here the black dots are the late stage materials. There's not a real obvious correlation between oxygen and carbon. But, we also haven't taken into account temperature/depth relations, which could give us some of the oxygen variation. We might be able to ultimately find a crude correlation, negative correlation between carbon and oxygen.

But, at any rate, we have interpreted this to indicate that there is no real obvious control of Pleistocene climate on the percolating water in the last couple of million years, and that calcite deposition is not restricted to a single climate state.

So, that was sort of the old work. More recently, we've been moving in the direction of micro-records of quaternary climate. And, obviously, in order to get a handle on quaternary climate variations, we need age resolutions that are at least on sort of a thousand year time scale.

We demonstrated that these minerals do grow very slowly. So, it requires that we sample them at much finer resolutions than we've done previously, which was probably on the order of hundreds of microns to millimeters in thickness.

So, we have used two approaches. One, ion microprobe dating, and then in situ micro-digestion. I'll talk about each of them. But, in each case, we've concentrated initially on this Sample HD2074, which is a thick coating on lithophysal cavity floor, probably gets upwards of 4 centimeters in thickness. We're at ESF Station 35+51, which is in the Topopah Spring welded, and we're approximately 270 meters below the land surface in the repository horizon.

First of all, Ion-Microprobe dating, we're utilizing secondary ionization mass spectrometry. We've chosen to do this at the USGS Stanford SHRIMP-RG in Palo Alto, where we generate a primary oxygen beam in this part of the instrument. We focus it to an approximately 40 micro spot, bombard our opal target, generate a secondary uranium and borium ion beam, which then gets detected, goes through a magnetic sector, several electrostatic filters, and ends up being detected at the far end of the instrument.

And, compared to standard methods, we do lose some precision due to the small intensity of the beams. We're only generating an amount of a very small active volume here. And, so, this translates to these very large pink air ellipses compared to the tiny little black dots that you see there, which are the air ellipses for our standard thermal ionization mass spectrometry data in the past. But, we feel that we gain accuracy due to the finer spatial resolution, and this is reflected in this isotope evolution plot in a closed system isotopic evolution, we should follow these curves, and you can see that we're doing that much better with our big red blobs than we are with our scattered little black dots.

So, in particular, we've looked at two separate traverses over two separate oval hemispheres. Outermost spots consistently are yielding dates of around 50,000 years. We have one spot here, Number 33, where we purposefully overlapped the 40 micro spot with epoxy on one-half and opal on the other. We got a date that was younger than the 50,000, outermost, 34,000 years. That tells us that even at that spot size, we're looking at mixtures of older and younger aged material.

And, then, as we proceed down into the interior of these bubbles, we get older ages. Basically, we're looking at about 400 microns for that series of dots, about 600 microns, and a total of maybe a millimeter's worth of deposition there, and our oldest model age is 1.4 million years, indicating that bubble took a very, very long time to grow.

We can then combine age-depth relationships and get average growth rates of about .6 to .7 microns per thousand years, which is the same as millimeters per million years over the last 1.5 million years. And, at this scale of resolution, analytical and spatial resolution, we are not seeing a real discernable variation in growth rate.

Also, these slightly slower growth rates are a bit less than the Tertiary uranium lead data that we've got for the whole coating, in this particular case, 5 microns per thousand years, or 5 millimeters per million years, and this kind of information is consistent with a shift to the increased aridity and decreased percolation flux that we might see in the quaternary compared to the Tertiary.

The other technique that we're using now is an in situ microdigestion, where we sort of coral the opal, and either using was dams or embedding the grain in epoxy, applying concentrated HF, hydrofluoric acid, directly to the outer surface, letting it sit there for a couple of minutes, and then picking it back up along with the opal that it dissolved, we're spiking it and analyzing it by a standard thermal ionization mass spectrometry technique. And, what we end up seeing is instead of the 150 to 230,000 year ages that we got when we digested that entire hemisphere, for the outermost surfaces, we're now seeing ages that range from about 4,000 to 12,000 years.

We can also do this microdigestion technique sequentially, and, so, we can basically peel apart layers, look at deeper values within a single hemisphere. We've done this in particular for one of the same hemispheres that we chose to do ion microprobe work on, and basically removed 22 microns of opal in a series of eight separate digestion steps, with each step removing between about 1.5 to 4 microns of opal. And, if we do the growth rate thing here again, we end up seeing ages that range from 7,000 to 37,000 years.

And, if we look at all eight analyses, they provide an average growth rate of .68 millimeters per thousand years, which is identical to the .69 millimeters per thousand years that we got from the ionprobe data, although we're looking at a very different part of the hemisphere. So, those two scales are very similar for the last 22 microns versus around a thousand microns.

And, if we look at it in a little bit more detail, we may find that the data define two different slopes with an inflection around 25,000 years. So, that growth rate is I think .35 microns per thousand years, and that's around 1.2 microns per thousand years.

We also tend to see regressions that indicate non-zero ages for the outermost opal. At zero depth, we have a positive age.

A couple of last slides here. Additional ion-microprobe studies that we're doing. We started some initial attempts to look at oxygen in late-stage calcite. We can also extend this to carbon and look for Devil's Hole type records. The problem is we've got to look very finely for them. We're looking for a Pleistocene climate signal, a nice squiggly line, and the initial data show a three to four per ml. range in oxygen, which is similar to what we see with our conventional analyses.

And, if you look real hard, you might convince yourself that we'll be able to piece together some kind of a systematic variation through time. We're going next week back to Palo Alto, where we'll try to do some dating on this opal. Right now, we don't have this constrained with any uranium series ages. So, we're still actively doing this work. And, then, we're also trying to develop uranium lead dating by ion-microprobe, with a colleague in Western Australia, Alex Nemchin.

And, as with the uranium series, we are seeing-- that should be a 20 to 30 micron spot diameter. Again, the results are less precise, but more accurate uranium lead ages for the same reason I described before, and we see outermost ages between .4 and 1 million years, with the growth rate calculated of about .92 millimeters per million years. 6 million year age for intermediate opal, and then 10 plus or minus 3 million years at the base. When we use all this information, we get slightly larger growth rates, 2 millimeters per million years, which, again, is consistent. The difference between the Pleistocene growth and the Tertiary growth is consistent with what we've said before.

So, in conclusion then, the minerals reflect some evidence for gradual climate shifts, especially from the wetter miocene and Pleistocene, to the more arid quaternary conditions. There's both differences in growth rates, as well as timing and compositional shifts for at least carbon that tell us this.

We know that there is slow, uniform growth rates, something on the order of 1 to 5 millimeters per million years, in the Tertiary, something less perhaps than 1 millimeter per million years, or a micro per thousand years in the Pleistocene, and these kinds of slow growth rates are consistent with the UZ hydrogeological system that seems to be buffered from extreme events and short-term hydraulic fluctuations. And, it also is evidence for long-term hydrologic stability of the unsaturated zone.

We also see that late-stage calcite has a stable isotope record that indicates to us deposition wasn't limited to only one part of the Quaternary climate cycle, that deposition was more or less continuous across that span.

We certainly know that very high degrees of spatial resolution are required in order to try to work out these Pleistocene climate signals.

Microdigestion dating implies that in fact UZ percolation hasn't been completely buffered from these kinds of variations that we see at the surface. And, at least based on our preliminary information, above-average growth rates, which we equate with increased fluxes, could be present during full-pluvial climate states. Our record in this particular case goes from around 37 to 20,000 years.

And, then, below average growth rates, which we interpret as a decreased flux in the unsaturated zone, may be present during the intermediate climate states between around 25 and 7,000 years.

And, then, we also have some evidence that perhaps interpluvial conditions, which we're experiencing right now, the percolation flux may be too low to exceed whatever seepage threshold is required to get free water into the cavity. So, that we've got depositional hiatuses over the last few thousand years in terms of both middle Holocene ages for the outermost microdigestions, as well as non-zero age intercepts for the regressions.

So, with that, we'll take questions.

PARIZEK: Thank you, Jim It seems like the mountain moderates the effect of these fans coming and going, as well as forming, and canyons being cut and rain coming and going. You don't find strong signal in your secondary minerals of that, although these little peels you're doing may turn that up, you're starting to show this with regard to the oxygen isotope data?

PACES: Right. And, I think we still have to admit that we're never going to be able to see an El Nino event well within the mountain, just on the basis of the analytical resolution required to see that time scale, but also they may not--we don't see any evidence that we have significantly different depositional ages, at least on thousand year time scales that we're starting to look at. So, we do see what we're thinking is moderation, some effect, but still a moderating effect by the hydrogeology.

PARIZEK: Parizek, Board.

I guess if you find there are gaps in the record on those thin peels, one interpretation that was no flow, another possibility would be some erosion or corrosion of those minerals, it could go either way. So, in terms of the episodic nature of flow, how would you deal with that?

PACES: We think, at least in terms of calcite, there's enough calcite in the system, in the soil zones, soil calcites, hundreds of thousands to millions of years old, as long as the water picks up calcium carbonate very quickly. It's very difficult for us to imagine a scenario where we're able to get water deeper than the mountain that's unsaturated with respect to calcite. So, I mean, it's not only got the soil that it's got to go through, but then along these pathways, there's plenty of calcite in the mountain, and we're not seeing major evidence of corrosion within the individual mineral deposits. We're not seeing the effect of non-deposition. And, our fastest growth rates seem to be associated with the wettest periods, at least so far. So, again, I don't think we're missing non-deposition because of too much water, if that was where your question was headed.

PARIZEK: Or at least changes in the quantities of water with time. Thure?

CERLING: Cerling, Board.

Do you see any hope in being able to quantify infiltration rates with your growth rates?

PACES: We have made some attempts at determining what kinds of percolation fluxes and seepage fluxes are required to get various different records. This has been a fairly crude scale at this point. Whether or not we'll be smart enough to figure out ways of making that translation between flux and growth, I think we can do it from a relativistic viewpoint with a certain amount of confidence. But, whether or not we'll ever be able to absolutely calibrate that scale is questionable.

CERLING: Cerling, Board.

I guess following on that, if there are zones that you suspect are sort of preferred pathways, do you find significantly higher growth rates in those zones? And, then, even following on that, do they then plug themselves up?

PACES: That's a good question, and I think we have the possibility of looking at focused flow. We know that the infiltration model has changed. I think Alan is going to probably tell us about the latest versions of the infiltration models. We now have a lot more water coming through washes than we did ten years ago. And, in Drillhole

Wash in particular, this is one of our line survey intervals where we see particularly abundant calcite deposition.

We need to go back and look at that more closely now, and see if, one, there are differences in growth rate, but also differences in particular, during the isotopic composition of these water, has a potential to be lower, if there's faster percolation rate. The uranium series systematics may be able to allow us to identify areas of greater and lesser flow.

So, I didn't include that story here today, but that certainly is possible, both within the minerals and within whole rocks and water/rock interaction and depth. Again, it's probably a relative record, and whether or not we can get an absolute calibration on it, remains to be seen.

CERLING: Thank you.

PARIZEK: Priscilla Nelson?

NELSON: Nelson, Board.

When you're doing your analyses, do you, we've heard a lot about what goes on in the lithophysae, are you also able to sample fracture surfaces, and is there a difference between what you observed for fractures?

PACES: Yes, we have worked with fractures. The problem is fractures tend to lack opal, and, so, it's much more difficult to get ages off of fractures. They tend to be thinner. We focused on lithophysal cavities because they're easier to work with. It's easier to get information squeezed out of them. But, the information that we have to this point, and, again, it's on a fairly crude scale, it's just that there aren't major differences in the ages of the outermost surfaces of fracture calcite versus calcitie in lithophysal cavities.

NELSON: Nelson, Board.

The difference between the fractures and the lithophysaes, what does that tell you, if anything, about what's going on with the slow moisture movement in the mountain? We have different mineralities, different thicknesses, different habits, what's going on?

PACES: We have a conceptual model. I don't know if we can prove this, but we have a conceptual model that fractures are generally steeper. Floors of lithophysal cavities generally dip gently 10 degrees, or so, to the east, whereas, many of these fractures are practically vertical, or at least very steeply dipping. So, that if water is moving down fractures, as film flow, it moves more quickly along fractures than it does where it allows, I shouldn't use the word pond, because we don't see any evidence for actual ponding of water, but flow slows down when the surfaces get close to horizontal, and that allows us to develop more mineral that is not available, and a gravitational control on the hydraulics.

NELSON: Nelson, Board.

So, this might have an impact on your prediction of infiltration, because I mean if you've got, or your correlation with infiltration, because you had two pretty much different things going on, it seems, on the fracture than in the lithophysae.

PACES: Well, we think they're linked, and there's no real way for us to imagine to get water into the lithophysal cavities than through fracture flow. If there was somehow water was coming out of the matrix and getting into and causing lithophysal cavity growth, then we would expect to see lithophysal cavities everywhere with material in them, secondary minerals in them. We don't. Secondary minerals only occupy a small proportion of all of the lithophysal cavities. So, we think that there has to be something to do with the connected series of fractures in a fracture network that's supplying the water that results in these deposits.

NELSON: Just finally, do you have a case where you've actually got lithophysaes, and be able to tie what's going on inside the lithophysae, with the fractures coming in? I mean, so you've got this whole picture?

PACES: You can see that relationship in the ground, but, again, it's difficult to try to peel these things apart. We probably don't have any situations where we could look at, in great detail, you know, the growth rates in fractures and how that changes in the lithophysal cavity. You see them at times coming into or leaving the cavities, but there are other cavities where it's not obvious from the exposure we've got on the tunnel wall, it's not obvious how water is necessarily getting in.

NELSON: This is an interesting point. Thanks.

PARIZEK: Van Genuchten.

VAN GENUCHTEN: I'm fascinated by your talk. I wasn't initially sure if you were actually talking about the fractures. I thought you were talking about the fractures, so this is not necessarily representative of all fractures in the mountains; right? I guess you must have seen quite a lot of these coatings. Are they pretty continuous? I'd like to talk more about fractures now. Are they fairly continuous, or if they are in fractures, are they more like point build-ups?

PACES: They can be fairly continuous, although it's common that they're patchy. One thing that we think is required is open head space in order to have air flow and liquid flux interact to form these things through either very slow amounts of evaporation, or very slow amounts of CO_2 de-gassing of the liquid. And, so, one thing that you can see fairly easily underground is a fracture that is tight, say, above or below. It opens out because of a wrenching differential movement, and you all of a sudden have centimeters, some centimeters worth of opening. There's no real obvious mineral coatings on the closed fracture, but as soon as it gets out to this open cavity, which may, you know, may go off in a third dimension, that's where we see these substantial build-ups of secondary materials.

So, the slope has a very complicated, in some cases, there's evidence for sort of fingering. We haven't documented that real well because we really are looking at a two dimensional view rather than a full three dimensional view. But, we think that this has to do with fluid flow as films in response to gravity, and then when you have an open cavity, you have the ability for independent migration of gas phase, and interaction between the gas and the liquid with our secondary minerals.

VAN GENUCHTEN: So, when you have very little deposit, you know, not necessarily the very recognizable larger species, but it still may significantly affect the hydraulic properties, I would say, of the fractures?

PACES: I think that's probably true. Sometimes these coatings are tightly cemented to the substrate, sometimes they're very loose, and they can fall down, especially some of these steeper fractures, it's common to see a breccia at the base of one of these things, where you've got fragments of coating that have dropped down, and now have been recemented by later calcite.

VAN GENUCHTEN: Now, you're talking mostly about calcite and opal, I guess. Have you seen any secondary minerals, or maybe even organic coatings? And, is there also, in a sense, a difference between closer to the top of Yucca Mountain, closer to where the soils are versus deeper in the mountain?

PACES: Yes, we have information on just calcite, silica deposits is a simplification. There are other mineral phases that have been identified. Those are certainly the most dominant. Fluorite is one that has been seen, and is somewhat controversial. With regards to vertical variations, we tend to see the greatest abundances near the surface, lesser abundances below the PTn. Again, I didn't show the full suite of information that we've got here. I was focusing on things that could relate to climate change. So, I don't know if that answers your question, or whether you want to take another stab at asking it.

VAN GENUCHTEN: No clay minerals mostly. It's mostly the calcite type?

PACES: There are certainly clay minerals, and in particular, clay minerals on fractures, but what we don't tend to see are clay minerals captured within these secondary hydrogenic mineral coatings. So, I think that we aren't doing a whole heck of a lot of rock weathering in this environment, even in the PTn with a lot of glassy materials. We're probably seeing little movement of aluminum, and other things, that are required to create clay minerals, except perhaps very early in the history of the mountain when temperatures were quite a bit warmer, and we were able to alter and transport those other ions much more effectively. So, we see manganese oxides, we see zeolites, we see clay minerals, but we generally don't see them incorporated into these younger secondary hydrogenic deposits.

VAN GENUCHTEN: One more question. You know, you correlate the growth of these minerals rather furious. Another scenario I always had in my mind, and I guess maybe it's wrong, is that also during dry periods, you can detect water evaporating from fracture surfaces, and it will be matrix water, you know, and then if it evaporates, it may leave some kind of a coating or precipitate behind. Would that we a plausible thing, too.

PACES: I think that there is a certain amount of fracture water, matrix water interaction that's going on. And, when we look at the isotopic compositions of pore water, we see compositions that look very similar to our fracture mineral record. But, again, we don't have physical evidence that indicates that matrix water is a dominant source for these mineral deposits. Otherwise, we would, since matrix flow is occurring pretty much throughout the entire unsaturated zone at some level, we would expect to see a uniform distribution, and not the sporadic distribution of these phases that we see. But, nevertheless, there must be some interaction going on. We have evidence that indicates--

VAN GENUCHTEN: Well, it would, I guess it would then evaporate more from the areas where you have the larger fractures, and you have much more air flow.

PACES: Right. I think that that's a key point, is this independently migrating gas phase may be a limiting factor as well, and growth rates may vary somewhat, because not only fluxes, water fluxes are different, but gas fluxes may vary from spot to spot, and that may give us some of the variation as well.

PARIZEK: Thank you. Parizek, Board.

I guess you were pursuing the colloids. Why wouldn't the colloids that were migrating down through the mountain be trapped in the secondary minerals? We've asked this question before. In the comments you make, you still can't say you found colloids sticking in the secondary minerals, other than in the case of the opal perhaps. That was a suggestion from the Nevada people at one point. Maybe that's where they end up.

PACES: Well, certainly we're talking about high silica here, and, so, there's no lack of silica available for movement. Almost every water that you find out there is saturated with respect to silica.

PARIZEK: How come opal only comes every now and then in your cross-sections? You only show a layer, and again, you show another layer, and there's some calcite in between. Is that episodic? Is that the evidence of episodic story in a bigger or coarser scale? And, then, if you plot up all of the dates you have for the opal, do you see gaps?

PACES: From some of the slides, there's clearly--

PARIZEK: Some breaks in there?

PACES: Right. And, we don't fully understand the system adequately to say why opal is common in some samples in some time periods, and more or less absent, completely absent from other places, and it's something that we wish we knew. We don't.

PARIZEK: Parizek, Board.

At one point, we saw some cross-sections that suggested there was some secondary minerals that were corroded out. This, again, may have been Nevada sponsored studies. Do you see any evidence of that, vapor phased minerals that disappeared? But, again, this idea that somewhere along the line, foods have gotten in there and chewed out some minerals through time.

PACES: Right. And, in particular, some of the bases, some of this material is tightly cemented to the substrate, as I said before, some of it is only loosely held, and it looks like there's evidence for corrosion. I think as part of an independent migrating gas phase, as you move gas in, it will respond to the thermal regime, so as you move gas upwards from hotter, warmer conditions of depth, to cooler conditions, it will condense at some place. And, in that sense, you'll get an undersaturated solution that could do some corrosion. That's how we prefer to think about those situations, rather than material coming from the surface that remains unsaturated through the whole mountain. We don't seem to see those records up higher in the section in these mineral coatings. That seems to be confined to the base. So, there could be some extra complexity going on with condensation, evaporation, saturation.

PARIZEK: Parizek, Board. One more question.

Do you have some sort of limits to where you think you're going to go with this? I mean, you're done with the peels, the little thin peels you're working on now. After that, do you recommend that you've got all you can out of this, or you're so excited about so many different directions you can't give it up? Do you see new leads? Obviously, the science has gone a long way, and you've made presentations to the Board many, many times, and we see a steady progress in the work you've done, refinements and refinements, and they've added understanding.

PACES: Right. And, I think we, as you well know, I think we have certain people to thank for continued interest in investing in it. This whole fluid inclusion controversy allowed us to continue to collect more information. And, I would say it's like so many things on this project, the more you learn, the more you need to know. We now also have evolved techniques that let us look at things in a completely different manner, and we would love to be able to do some more of this work, and we have funded projects to look at some more of this. How long that will last, and how much we can get done is hard to predict.

But, I certainly think that we have to do more than what we've already done. We need to demonstrate that that trend we saw for one sample in one spot is extrapolatable to different parts in this system. We need to start to understand a little bit more the differences that occur in the Tiva, where air flow is much more active than beneath the PTn. And, so, there are a number of things that we could continue to do, and probably learn a substantial amount more about the system.

PARIZEK: David?

DIODATO: Diodato, Staff.

I just wanted to follow up on Dr. Parizek's question about the colloids, colloid facilitated transporting in the unsaturated zone is something that people are thinking about. In your observations, you don't see any colloids anywhere in any of these minerals captured. So, that suggests to you that even though there's clay minerals that occur, they're not captured in these minerals. So, my question is in nature in general, can these minerals, as they grow, incorporate exogenous materials like that that would fall in as the mineral is growing, and, you know, in other places, you would have a chance of seeing that sometimes, or does that not happen in nature? Is the nature of these mineral growths such that they could never incorporate that?

PACES: Well, that's a good question. And, getting back to the question about the explanation for why opal occurs in some cases and doesn't occur in others. We have a number of really fascinating secondary electron microscope images where it looks like calcite does not want to touch opal. There's something about that interaction that is repelling the calcite. They're growing simultaneously, it's very clear of that, but we haven't really hunted for colloids. If, by colloids, you mean can we find evidence of clay minerals in these, we've done chemical, we've analyzed them for their full suite of major and trace elements, and they're very clean calcites, they're very clean, outside of uranium, there's very little in opal.

DIODATO: But, just in general in nature, could you have, say, montmorillonite, something like that, in small particles preserved in some kind of a silica mineral, an opal deposit, or something like that? Have you seen that? Are you aware of that at all?

PACES: Like I said, where we have looked at the compositions, you know, we see trace amounts of aluminum, but not more than that. So, we don't see, obviously, on a microscopic scale, you know, maybe once you get down to a nanoscale, we could easily miss it. But, at least on a micro-scale, it certainly isn't obvious from our studies.

DIODATO: In your career, you haven't seen these things?

PACES: No. And, it could be that you're leaving much of this stuff, you know, you weather the PTn, the glassy phase in the PTn, and you leave the clay minerals up there, and that would imply, I suppose, that it's not being transported further down. Also, you could look at the fractures themselves for evidence of clay minerals, but what you wouldn't get there is when were they established.

DIODATO: Well, the question is the mobility of colloids, if they're mobile at all.

PACES: And, that has not been a focus of our studies.

DIODATO: Thanks.

PARIZEK: Rien?

VAN GENUCHTEN: I have one more question. If you take a step back and you look at all your data that you have collected from the mountain, do you see any evidence that some of the flow pattern may have changed over the years, not just from dry to wet periods, but also I guess tectonic activity?

PACES: Again, I don't know at what level we can answer that question. But, certainly we were surprised, once you establish an active flow pathway, it looks like you can maintain that flow path for millions and millions of years, 10 million years. We've got single records. Again, I think initially, we expected to have to hunt, you know, we'd see a 10 million year deposit, we'd see a 3 million year deposit, we'd maybe come across a Pleistocene deposit, but we would, you know, really have to look hard.

On the contrary, we see, wherever we look at this, we seem to see a very long history of deposition which implies stable flow pathways, stable deposition of processes, everything seems to point towards hydraulic stability. And, true, you know, tectonics happens, and we might make new flow pathways, and I think we do have evidence that not all basal calcite is 10 million years old, or 12 million years old, or .7 million years old. But, once you establish that pathway, it seems like in general, we can maintain that flow pathway for a very, very long periods of time.

VAN GENUCHTEN: Can you, putting it all together, can you trace where those pathways are then from the top down?

PACES: On a crude scale, I think we can. And, right now, we've also got funding to take a look at trying to identify flow paths, preferential flow pathways by looking at water/rock interaction with whole rocks. So, rather than these secondary minerals, we're actually looking at fracture surfaces and more fracture than less fractured rock, to see if there's differences in uranium series disequilibrium in particular, but other elements, and isotope systems, as well.

And, we're looking at a couple of fault zones in particular, Solitario Canyon Fault Zone, I'm sure some beautiful development of clays and bleaching and leaching. The question is is this largely a 12 million year old phenomenon, or is it a result of focused flow in that fault zone over the last 12 million years. And, we do have funding to address that situation with uranium series disequilibrium. We've looked a little bit at the Bow Ridge Fault, very close to the surface in the tunnel. And, yeah, we can see those differences. It looks as though fractures can focus flow, and we can find physical and chemical evidence of that. So, yeah, it depends on how hard we want to look, too, how much detailed information we can get.

PARIZEK: Jim, we thank you very much for your comments. And, as always, there's a lot of information that's been very helpful, but we do need to allow time for the last speaker, Alan Flint, before the lunch break. And, judging from the number of viewgraphs, he'll need every second of available time. And, this is not, by any means, evidence of unstable science. It means that the program has allowed a lot of discovery that we're going to discover from his presentation. But, Alan got his Ph.D. in soil physics from Oregon State University in 1986, and since that time, he's been working with the USGS as a research hydrologist for the Yucca Mountain Project in Mercury, and later, in the California District at Sacramento.

FLINT: All right, thank you. I do have a lot of slides, and I will talk real slow.

Basically, a lot of what I'm going to present has to do with about four major papers that have come out in the last couple of years that I have written with Lori with Bo, with June Fabryka Martin and Ed Kwicklis, moving authorship around, but a lot of the ideas we worked on together over the last 10 or 15 years.

This started, the evolution of the conceptual model, and how we got here, started with an NRC Council Panel that I was on with Rien van Genuchten, and we sort of worked through the development of our conceptual model. We came out with a Journal of Hydrology article on the evolution of the conceptual model that NRC let us publish that had some lessons learned in it. We did a Reviews of Geophysics paper on the hydrology of Yucca Mountain. These were invited papers that we were asked to do. And, then, Hydrogeology Journal finally was a paper on a comparison of all the different methods that have ever been used to estimate recharge at Yucca Mountain and how these compared in the calculations. And, those papers are all available in more or less a PDF format, and I've provided some of those.

This is one of the papers that was in Reviews of Geophysics. We were lucky enough to get on the cover and got a write-up in Science Magazine as an editor's choice for Geophysics for that particular year. And, it shows the infiltration map of Yucca Mountain that was developed in '96.

And, this is the conceptual model of flow and transport in the fractured vadose zone, quite a few papers in here on flow and transport, and the one we did on the evolution paper, and also some very good introductory material on developing conceptual models.

This is that example of how one would put conceptual models together. I put it in the overhead. This is something that came out of our panel. But, I think really important, when you look at this, if you can only see three things in it, besides having your problem stated and data, is that you have a conceptual model, a mathematical model, and then model calibration that feeds back into itself. And, it's this combination of numerical and mathematical model that become so important, and that's what we were missing in the early conceptual models of Yucca Mountain, is we did not have good mathematical models to try to test some of these conceptual ideas, and that's where some great progress was made once we put that together.

In terms of the early conceptual model, where were we? Between 1983 and 1990, a lot of work was done on conceptualization, but this is some basic information, if you look at this, 80 per cent chance that the flux was less than a millimeter a year. That was what we had gained by about 1990, 1991. That's what the thinking was, and that was coming from a series of conceptualizations.

We had a lot of information. We had some deep boreholes. We could do potentiometric surfaces for the water table. We had our shallow neutron holes that Dell Hammermeister had started. We had a lot of surface geologic mapping going on. We had some meteorology studies looking at rainfall. We had geochemistry and hydrologic properties of rock core, giving us our fire insights into the mountain itself.

The early conceptual models did identify water as a critical parameter. They described the simple geology and the hydrologic framework. They identified the relevant hydrologic processes, and the consequences of hydrologic flow. There were a lot of conceptual models that all had about the same kind of information.

This is one of the first conceptual models by Scott and others. Mike Chernack was a co-author on this. And, this model may be the closest to the model we have today. And, very basically, all the models are very similar. Tiva

Canyon, they were estimating about 3 per cent of the rainfall becomes net infiltration. We have fracture flow, then matrix flow through the PTn, then fracture flow again in the Topopah Spring, and then either some lateral flow or vertical flow through the Calico Hills, very, very simple conceptualization, but it was the first start at putting something together of how the system worked.

The difference between this and the next model, which really dominated the thinking of the project for the next ten years was going to be with the Montazer and Wilson. This is Roseboom's early one when he was recommending the unsaturated zone, and looking at the differences between the two, just simply for reference.

So, this is the Montazer and Wilson picture of things. But, the main difference here is that Montazer and Wilson had very small flux. They had most of the infiltration becoming lateral flow, and not going through the Topopah Spring across the top of the PTn, and they only had matrix flow in the Tiva Canyon. So, fluxes were on the order of a half a millimeter a year, a very important concept, and very dominant in the thinking for a long time about Yucca Mountain.

This is DOE's conceptual model, which is basically Montazer and Wilson's conceptual model. But, one of the things to note is that there is a lot of this--the flow through here, a lot of lateral flow across the top of the PTn. That was something very dominant in these particular models, and flow along the Calico Hills zeolitic rock.

So, there were four major components that really influenced the thinking, and they didn't necessary move it forward, they might have held it at a certain place for a long time. They had to have a fully saturated matrix to get fracture flow. The overall flux was low. Only matrix flow occurred in the Topopah Spring welded units, and most of the net infiltration was diverted by the PTn. This is what's in all the papers up through the early Nineties, is how the system behaved. Again, no numerical model in particular that we were using at that time.

This is that hypothetical relation between the permeability and matrix potential for the double-porosity model, which is what linked the two together. This came out of Montazar and Wilson. This is what we started using where we had to have the fracture matrix and equilibrium, and the wetter we could get it, then we could start fracture flow.

This is Wang and Narashimhan '85 concept of the only way you get flow across fractures, but you still had to have the saturated matrix to get fracture flow to occur. And, these were very big issues in the thinking of the Project.

I'm going to jump forward to about 1996, when Susan

Altman put together a very nice list of different ways to conceptualize fractures. This is when we advanced our conceptual model. Where we were in the early years, is back in here. So, early on in the project, this was where we were running our modeling and our thinking about how the system behaved. It wasn't until later that we started separating fractures and matrix. It became an important contributor to our current thinking.

So, what we did to get our current conceptual model working, and this is our mid 1990s paradigm shift in the way we were thinking, is we finally got our three dimensional site-scale numerical model, a major advance on how we were going to think. Another thing that happened that I think was the most important thing was the spatially distributed high infiltration maps that we finally started developing. Along with this, the higher the infiltration, the less lateral diversion in the PTn. We started finding evidence of fast fracture flow in the Topopah Spring, and then a decoupled fracture flow. That's a very important modeling breakthrough, is this decoupling. Robinson and his group had done some separation of properties between the fracture and the matrix that started to allow higher flows to go through.

The biggest problem we've had was the high infiltration rates in all of the current models at the time, and up until about in 1993, '94, those high infiltration rates had to be scaled to less than a millimeter a year, no matter what they were. 10 millimeter flux, we put on 50, they all had to be scaled to work, because they completely saturated the matrix, because of the matrix/fracture interaction. Cliff Ho came up with the idea, which I think was a real important point, that decoupling the fracture so you only had about a four order of magnitude coupled between the fracture and the matrix, so you could have the high fluxes, you could have fast fracture flow, and you could keep the matrix still up at 90 per cent saturation, that was a major advance.

But, I think it was Bruce Robinson and his group that really pushed the idea of making the modelers start to think about these higher fluxes, getting away from scaling to a millimeter a year, and starting to think how do we get 10 millimeters a year in the model. That made a major difference.

This was the 3-D site scale model. It was based on two concepts. One, infiltration zones about the mountain, and the other was faults. So, these were the grid cells we put together. This model came out of a meeting between LBL and USGS in I think about 1991, and 1992, this was the model, and then Lori and I published it in '94 because it ties into our infiltration map.

And, this was the first infiltration map we produced in about 1994. It's based on Darcy flux calculations from core and neutron logs that we had in all the major hydrogeologic units. It's only matrix flow, no fracture flow is considered in this. But, we have an overall flux of a little over a millimeter and a half a year, which is above the half millimeter everyone was thinking we were going to have in these rock units. And flux is over 13 millimeters in the non-welded units in the PTn. They were very wet, and they were high permeable units. So, using Darcy calculations, we came up with this particular map.

Then, by 1995, David Hudson and I did some statistical analysis on neutron borehole data. We came up with the correlation between soil thickness, between rainfall, between the topographic areas, and came up with the first major map of infiltration, with some fairly high values.

In 1996, we used our numerical model to put into the model, evapotranspiration, more of the salt physics approach rather than statistical approach, and came up with the map on the right, which is the one that became the first major infiltration map that was put into the system.

And, I'm going to talk a little bit about the development of the infiltration model, because I think that's an important point to this whole process of understanding the behavior at Yucca Mountain, and how it's going to change with climate change. So, we're going to look at the development of a conceptual model and how we got there.

Net infiltration is a precursor to flux. It's what we need to start with. It's water entering the soil. The net infiltration is water gets below the root zone. You need to know that to know what recharge is going to be. Percolation is just continued drainage. And, then, recharge, although it may be delayed by 5,000 years through the unsaturated zone, it's what finally makes it to the water table. And for most cases, net infiltration is going to become recharge, unless you have lateral flow to a perched layer that's going to evaporate somewhere else in the spring.

The factors controlling net infiltration: precipitation, number one, the soil thickness is very important, soil porosity and drainage characteristics are what are going to hold the soil moisture in the near surface where it can be removed by evapotranspiration. Deeper soils have a little bit more storage room.

The bedrock permeability is important. High permeability bedrock is going to be able to allow that water to drain in faster. Low permeability is going to hold it near the surface for longer. And, then, evapotranspiration is going to have an important component, especially when you start looking at the north end of Yucca Mountain, and you look at the north facing slopes at Yucca Mountain, very different here. We're in the transition between the Mojave and the Great Basin. The north facing slopes, more like the Great Basin vegetation. The south facing slopes, more like Mojave vegetation. And, those north facing slopes are going to have higher infiltration rates, especially when we go to the north where we get more precipitation.

So, a conceptual model of net infiltration is that this arid climates make infiltration infrequent occurrences. It doesn't happen every year, and it doesn't happen everywhere. Wet winters allow the saturated conditions to exist at the bedrock interface under shallow soils, which is what's going to get water below the root zone. The deep soils and non stream channel soils have sufficient water storage capacity to retain most of the precipitation. This is the reason arid climates are what they want to use for nuclear waste burial for low level nuclear waste under deep soils. Deep soils hold moisture, very little recharge. But, runoff accumulates enough water in channels to allow for infiltration of water in these channels that can get below the root zone so we can have net infiltration below channels.

This becomes, in response to Jim's sort of question, things like Drillhole Wash, under current climatic conditions, are not nearly as critical as under past climatic conditions. Right now, Yucca Mountain is likely more dominated by flow over the whole large area, but under other climatic conditions of glacial periods, the wash has become the major contributing factor, which is why I think they find more of the calcites under the wash, not because of current conditions in infiltration, but because of past conditions.

And, this is our conceptual model that we put together. All the terms are in here. But, the important thing to look at here, if anything, is that under shallow soils, the zone where you get water to to become net infiltration is a lot closer to the surface than under deep soils, because these deep soils have deeper rooted vegetation. We've seen roots down to 6 meters of creosote in Fortymile Wash. So, that's an important component to the conceptual model.

I'm going to show two examples of neutron holes that we used to help understand what's happening. And, the reason I'm going on infiltration is because all the recharge that's going to occur at Yucca Mountain, for the most part, is going to be determined in the top 6 meters. Once it gets past the top 6 meters, it's going to become an unsaturated zone flow issue, and no longer a question of infiltration. That's water you're going to work with.

So, two neutron holes, and one in the lower part of Pagany Wash, and N15 in the upper part of Pagany Wash. Here's an example of N1. This is depth versus time from 1984 to about 1995. We're looking at water contents in the wash.

What's interesting to see is these features where it's getting wet, and it's going down and over, which is movement with time. That's a wetting front moving down over a couple of weeks to a month or two in time. And, we see several events. Then we go through the early drought period, and then we have here, we're in 1990 now. In 1990, remember, we're thinking there's no flux at Yucca Mountain, because we're out there and there is no flux at Yucca Mountain. It's not even raining out there. It's the driest conditions you've seen.

Then, we had two El Nino years, and then finally, the 1995 major El Nino year. And, what have we discovered? And, we hadn't had the ability to look at this data in this way. But, once we could start to look at it this way, then we realized what happened was back in 1984, there was a major runoff event from another El Nino event that caused the wetting up of the entire profile, which ended up drying out over the next six or seven years.

So, now we can see what this historical view was of how the system was behaving, and it's very interesting I think to look at that in that light. But, you can see that for the most part, these major events in 1992 and 1993 did not cause net infiltration. That water dissipated in the root zones, and it wasn't until we got a major influx in '95 that we got infiltration.

This is a look at a shallow soil now. We only have about 70 centimeters over on fractured bedrock, very low permeability matrix, but high permeability fractures. Below that is very high permeability matrix rock. And, so, what we see is an influx in the 1993 El Nino event and the '95 El Nino events, where we got big pulses of water moving down through the fracture system. You can't see it with our neutron approach in the dense rock because there's no matrix imbibition. But, once it gets down into the more permeable rocks, we can pick up a lot of this moisture content, and we can see it moving down with depth, and then time to the right. So, we're starting to see some pulses.

Now, we're going to calculate how much water is going to be in here. This is going to be our first calculation of net infiltration. This is well below the root zone.

So, those pulses you can see in the right axis is the flux in millimeters, rather than seeing an average of 10 or 20 millimeters a year, what we're seeing is 200, 300 millimeters over a very short period of time, because we had a very, very wet set of conditions.

If we look at the in between time from '93 to '95, this profile is slowly draining out of the bottom, and we can see that. If we plot that up and put a line through it and calculate the slope of that line, it's about 20 millimeters a year. So, that's the drainage through that welded tuff down at the bottom of the profile. So, this is one way to calculate flux.

Another way, independent of boreholes, was the matric potential measurements we made, and a profile about 10 meters away from the borehole I just showed. We're looking at a 1995 condition in which we got our instruments in about a week or so before the major El Nino rainfall event that caused most of the flooding and the deep percolation. This data started early in that, but I don't have it here, but what we see is that we see near saturated conditions at the tuff alluvial contact, and even at about 30 centimeters, we see near saturated conditions, which means we had about 30 centimeters of standing water at the tuff alluvium contact. With that information, we can calculate a flux using the water retention curve for this particular soil.

This is change in water content for that profile. An evapotranspiration rate at this particular time was about maybe a millimeter a day, at most, and, so, we're seeing fluxes, and this is a fairly flat surface, on the order of 10 millimeters a day infiltration. One, it tells us there's a lot of infiltration due to this process, and, two, it tells us the rock permeability is high. These are higher numbers, almost by an order of magnitude, than what we were using on our original infiltration model. Whether that makes a big difference or not, I'm not sure. But, everywhere we've made measurements in detail, we've found about that increase. So, we can calculate a flux, we get about 200 millimeters out of this process in this particular calculation for this data set.

Just to show this over time, this was the early time that we started working with in here, and then what we see, and if you just look at this one green one, that's the tuff alluvium contact, it gets fairly dry, vapor dominated flow, these plants can take up to about 60 bars, so we have vapor flow even to that depth, and equilibrium at the near surface with the vapor, but we only see two more events in which we have a possibility of net infiltration. These are El Nino years, and they're positive Pacific decadal oscillation. And, the study I've been doing all over the desert southwest, negative Pacific decadal oscillation El Nino years are very insignificant in terms of recharge. So, it's not just El Nino, it has to be in the positive phase of the PDL.

But, we don't see that interaction, so we don't have wet enough conditions in the fractures, so we're forced to go only with matrix flow, and you're not going to get matrix flow at an interface of 100 bars to any consequence.

Are there observations that support these high fluxes? Darcy calculations in the PTn we did, there's tritium, carbon-14, thermal profiles. In one of the papers that I talked about that we published was on a comparison of all the different methods in estimating recharge. Here's an example of the thermal profile that we used to calculate a 10 millimeter a year flux, and this is mostly through the Topopah Spring Unit, or a 1 or 2 millimeter flux in different boreholes, we had different values.

How do these correlate with the infiltration model itself? This is an example. The net infiltration values, I think it's a reasonable correlation, one of the other things this suggests is what I think is a lack of major lateral flow in the PTn, because where we have high infiltration rates at the surface, we have high fluxes in the subsurface for the most part. There are a few exceptions in this case.

We did an analysis in the north ramp, where we had outposts that we could drill boreholes down. I had these put in and instrumented to measure water potential, so we could go across several layers and no what the water potential is, know what the core properties are on saturated hydraulic conductivity properties. We've calculated fluxes, vertical versus horizontal fluxes for this area, to see if we could support the high fluxes.

We did an analysis, and this is in a paper that Lori published as part of her Ph.D. dissertation on lateral diversion of the PTn, using Darcy flux calculations. She calculated about 8 to 15 millimeters of vertical flux in those two boreholes you saw, and less than 1 millimeter of lateral flux between two of the layers that she saw in that particular analysis.

Another example of looking at possible lateral diversion, there's two things to look at here. The boreholes, the yellow dots, the area of those yellow dots are going to be used in calculating the estimated net infiltration range. And, then, the cross-drift across the repository in terms of what the water potential is in the cross-drift versus what the infiltration map says. So, those are the next two things I'll talk about.

One, matric potential in the cross-drift versus the distance along the cross-drift on the left axis, and then on the right is model net infiltration. Where the infiltration is high, where we model it high, the rock is at its wettest, less than 8/10ths of a bar. Where the infiltration rate is low, the water potentials are up in a bar and a half, or higher. So, more infiltration, wetter rock; less infiltration, drier rock.

And, this is an example of the chloride mass balance method. The range of the infiltration calculations, those dots, versus chloride mass balance, another indication that there are high fluxes, and that there is little lateral diversion.

And, this is the summation of all the methods we used. Important thing, point measurements to the left, large scale to the right. The point measurements are going to be located in places where you're going to have high and low fluxes. So, you expect a big range. The larger the area you're investigating, then the lower the range you're going to get, because it's going to be an average of a lot larger area and a lot different time span.

As we did this analysis, we also calculate that we go from the surface to the subsurface, we get more and more Pleistocene water in the mix, in the subsurface unsaturated zone, and Pleistocene estimates on the order of maybe 20 to 40 millimeters a year, versus current estimates of around 7 or 8 millimeters a year, which is described in the paper.

So, beyond net infiltration, what happens? Unsaturated flow in the UZ is vertical, for the most part. Gravitational gradients dominate. Lateral flow in the UZ occurs under locally saturated conditions. If you have lateral flow, it's usually because of half layered barriers. Fracture flow initiated in the near surface can move quickly, less than 50 years travel time, usually to the PTn, based on isotope data.

Matrix flux in the PTn dampens seasonal and decadal pulses of water, except for faults, and it may increase travel time. Probably 90 per cent of the travel time is through the PTn. Vertical fracture flow in the TSw, lateral flow above the zeolitic Calico Hills, and recharge occurring through major faults. This is sort of where we are. And, this is a conceptualization of that in sort of a--as a picture of the same thing I just said.

I want to go back to one thing here. One thing I want to point out, and I think this is an important key. The fault itself can provide direct downward flow. These are our fast pathways through the PTn. Very little of the water, I believe, is going through there. It's a very small contributor in most of the unsaturated zones. Where the faults are the major contributor in flow is where they provide an opportunity for perched water to enter into the saturated zone. Most of the flow that goes through faults is in this very small area. Up here, they're not very significant, but they do bring us fast pathways, part of the conceptual model we have to work with.

And, this is just an example you've seen before with chloride data, where we have bomb pulse isotopes located in faults. This is in the Topopah Spring under where the PTn was faulted. So, an important contributing factor in our understanding of how the system behaves.

Our current conceptual model, which you'll probably see a little bit later, was based on the site scale model. And, if we take the infiltration map and convert that into a flux at the water table, we see most of the flux going through the fault zones. So, this is just an example of how this zeolitic Calico Hills has altered the flow, but that's below the repository, not above the repository. I still think a lot of the flux through the repository is very similar to what we see in the infiltration.

Lateral diversion. Just a couple of examples from something that's new. This unit has largely been known as location of capillary barriers. The modeling exercises repeatedly support the concept that PTn is a lateral barrier, but we believe, Lori and I, and John Selker, the models have typically used idealistically geometry and large contrast in properties. We think the models are not correctly representing the PTn.

The early observations of high saturation, as we can see over here, suggested this showed lack of strong property contrast, except that the bottom is the PTn. And, so, we used analytical solutions to look at whether or not we could get the lateral diversion.

The equation of Ross, it's described in detail in the paper, it's just a Darcy's log calculation between two different media, contrast and core sizes, and then we have downward flux right in the (inaudible), and the permeability differences.

Diversion above the PTn. The fewer layers you have, the more diversion you get, very simple. If you want to have lateral diversion, don't put many layers in your model. If you put more layers, you're going to get less lateral diversion, especially if you're using what we believe are realistic properties, because the contrasts are very, very gradual. We've published a couple of papers on the PTn, not just here, but in other papers describing the PTn in detail, and, the more layers, and we think these are real layers.

Diversion within the PTn, even if we use a five layer model, we can get a small amount in two locations. It may not be a major contributor if we start to look at the multiple layers that exist.

And, then, at the base of the PTn, and there's a lot of information here, but basically, if we use what we think are typically and unrealistically used properties, we can get diversion, although little more than 200 meters of lateral diversion. If we use what we think are more realistic properties for that transition at the base, we don't get lateral diversion.

And, there are some other issues, and these are idealized geometry, not just the properties may be more realistic, but in the real world, I think there's a lot of inconsistencies in the top of the Topopah Spring that's not going to allow lateral diversion.

So, potential on the basis of the interpretations. We think the early conceptual models did not consider the scale at which the mechanics were in place. And, we don't have date or field observations that corroborate this, we don't think, to any great extent. And, the calculations and field data support the conceptual model of small localized lateral diversion, but large scale fluxes through the PTn.

Just a quick thing on some fracture characteristics. There were some detailed measurements done in the ESF. The fractures may actually exhibit this multi-hump component, and the small fractures may be able to carry higher fluxes in potential equilibrium with a locked matrix. That's just an idea that we're just now working with.

An example of the different sized fractures that are calculated using the method of Kwicklis and Healy, so these are the 25 micron fracture, 125. These were the two modeling fracture sets that LBL used, quite a bit different than these different fractures. But, we keep that in mind. And, then, this is the flux rate for the potential of the matrix, and then what we would estimate the flux rate. And, so, we can't see an equilibrium occurring between the two.

One of the measurement points where the fractures are highlighted in the red lines, and you can see a data set, conductivity using a potentiometric (inaudible) versus potential. And, the character that's kind of interesting to see is we might be able to see that we're using higher fluxes, higher fractures, 125 micron. As we get down here, we would expect it to drop off, but it continues on, because it may be moving into the 25 micron fractures.

So, we may have a series of fracture sets that the water is flowing through. And, we can actually keep moving down with different size fractures until we get to a 2 1/2 micron fracture that can carry the flux and can be potential equilibrium for matrix, kind of an interesting concept. But, I think we need to think in terms of how these fractures really behave, which I don't think we've done as well.

Okay, final thoughts and lessons learned. Model development must have a clear statement of the problem, and identify the technical objectives. You can't say, well, is Yucca Mountain suitable for a nuclear waste repository. We can't answer that question. You can ask the question how much water flows through the fractures, or how long does it take to get to the water table. Those are the kind of questions we can answer. You need to ask those questions up front.

A variety of alternative conceptual models need to be formulated on fracture flow, fracture/matrix interaction, all of the different concepts. We kind of got stuck on two or three, and we used those for about ten years. We need to be working on other ones.

Absolutely, numerical models have to be developed concurrently with the conceptual models. You've got to keep these working back and forth. But, one thing to keep in mind, if the numerical model does not have the concept in it, it's not going to tell you that's it's an important concept. So, you've got to make sure you remember that. The data gives us more insight than changing the conceptual numerical model, but the conceptual numerical model gives us insight into what data we should expect to see. So, that's a very, very important key. For a long time, we couldn't get high fluxes through the mountain because we had a numerical model, but it had the wrong concepts in it that had to be fixed.

Evaluation of the conceptual model should rely on consistency with independent lines of data, and robust model development depends on extensive high-quality data sets at different spatial and temporal scales. It's very different. You can't look at neutron log data and say, well, that doesn't match the data I have in the subsurface, because it's a 5,000 year travel time difference between the two, and there are different processes and different space scales. You've got to keep that in mind.

Summary. The early models had low flux, extensive lateral flow in the PTn, and no fracture flow through the TSw. The current model has high flux, 5 to 10 millimeters a year, with over 80 millimeters in some locations. Matrix-dominated vertical flow in the fractures, matrix PTn, fracture dominate in the TSw, and vertical matrix-flow in the vitric rocks of the Calico Hills and the Prow Pass, with extensive lateral flow above the zeolitic boundaries in those units.

And, I know where the conceptual model was in 2001 and where it may be a little bit different now, and I'm sure Jim will talk about that, is in this idea of lateral diversion in the PTn. We think that lateral diversion can be calculated in the numerical models if you don't use the properties that we think are most consistent with what we see in the field, and that's something that I think needs to be discussed, perhaps a little bit more in a little bit more detail.

And, then, within these few concepts, we've made significant strides in addressing the major issues on the behavior of Yucca Mountain. And, this was true up until 2001. I'm not going to say it's true now, but it was true up to then.

The conceptual model we have today evolved over 20 years through an integrated scientific approach. We had highly motivated and creative scientists from a variety of disciplines and organizations that were provided a work environment that fostered quality technical interaction. That interaction was very, very important. I'm not sure if it still exists the way it did back in the late Nineties, but it was an important component to our work.

And, then, finally, I couldn't think of everybody that I acknowledged, so I just acknowledge people that I have actually published work on about Yucca Mountain. So, this is the list of people I've worked with.

I'm sorry, I did talk faster than I thought.

PARIZEK: Thank you very much. Well, there was a lot of material there, and we appreciate the overview, I mean the kind of historical run through so many of the bases for the change. Ron, I guess the first question?

LATANISION: Latanision, Board.

I'm always intrigued by the opportunity to look at things that I know nothing about, and try to interpret them in the context of things I know something about. And, this is a great example.

I'd like to turn to your slide that shows the Darcian flux calculation. I don't know what number it is. That's it. You just passed it. That expression looks very much like, shall I say chemistry Fick's first law of diffusion, where Q would be equal then of a flux.

FLINT: Yes, it's almost like Ohm's Law, too.

LATANISION: That's a flux, K is an effective diffusivity, or permeability.

FLINT: It's conductivity, and then there's a gradient.

So, a gradient, a conductivity.

LATANISION: Now, when you apply this in terms of the solid state, the implication is that you're dealing with a steady state diffusional phenomena.

FLINT: Right. This is assuming a steady state condition.

LATANISION: And, are those conditions conceptually consistent in terms of having a constant gradient, and an unchanging concentration with time? It doesn't seem to me to follow.

FLINT: Well, this calculation is made within the PTn, and in the deeper part of the PTn, and I think most of us are convinced that the PTn has an incredible moderating effect on climate change. And, the deeper down in the PTn, we're looking at more steady state conditions.

LATANISION: But, I mean, the implication would be that DPDX is constant. I'm sorry, the concentration gradient, or chemical potential gradient is constant.

FLINT: I mean, it's constant--I mean, it's measured in this particular location, the measurements have been in for a year or two, so we're in equilibrium with the rock itself. So, in terms of measurement, we think it's not a problem. And, in terms of how fast it's changing, I'm not sure, the evidence we have over maybe ten years suggests that it's not changing very fast at all. So, that calculation in this point in time, but that's what it is, it is an issue that if you were to come to a different place on the mountain and look at a different place, you would get a different gradient, without question. The spatial gradient is going to be very, very, variable. Under this location, this is what we got. If you went under the PTn, under a deep alluvial, you would find it much different than it is today, but we don't have that opportunity. We only have the opportunity where the ESF crosses through.

LATANISION: But, I mean, the affective point is that you're treating this as a steady state.

FLINT: Yes, at this calculation.

LATANISION: I mean, what follows then is a trivial question, but the unit you used to express flux are millimeters per year.

FLINT: Correct.

LATANISION: And, in a chemical transport phenomenon case, you would talk about something like moles per centimeter squared per year?

FLINT: Yes, there's different ways to make the calculation, but it's sort of just an average.

LATANISION: Millimeters per year sounds more like an infiltration to me rather than a flux.

FLINT: Right. I mean, you could put it into 3 millimeters cubed per square millimeter, and do it that way, per year.

LATANISION: But, it is a flux you're talking about, not infiltration rate?

FLINT: Yes.

LATANISION: Thank you.

PARIZEK: Priscilla?

NELSON: Nelson, Board.

I'm going to maybe put some of your comments both on paper and made here into the context of--this may be a confusing question, so I'm going to just talk it through. We heard from Jim and previous speakers the idea of fast paths being located in the same place perhaps through time. In the sense of decoupling the fracture flow from the matrix flow, it seems to me that it might be linked, because where the fracture flow is may actually have caused a modification of the fracture surface such that it is decoupled from what's going on in the matrix in terms of precipitation, or something else along the fast path that represents a decoupling.

FLINT: I guess I tend to look at, since I work on the surface and have done so much work on the surface at Yucca Mountain, I see this huge variety of infiltration rates, and I see a huge variety of processes. If we were to have some value, I'm using my hands, and say that under current climate maybe we have some rate in which the matrix, the near surface, shallow soils, side slopes, ridges, are about here, and I see the washes being down here, as we go through climate change, we move that up to where the washes become more critical. And, the washes are very localized. And, those pathways are there because the washes are there, and the water, the infiltration rates are there. And, so, the pathways are created where the infiltration rates are the highest.

And, so, I think that these pathways that we might suspect that we would find are related to, one, the tectonics and the topographic features, the faults and the washes, and the other is the infiltration rates, which don't change that much. They can change in quantity, but they don't change in where they're going to occur. So, we're going to see the calcites in the same place all the time. They're going to see them under some of the major washes where we have high infiltration rates, under different climatic conditions than today. I think that's something we can see in that sense.

On a larger scale, I think we're going to see these differences in where we're going to find calcites, rather than uniformly distributed. I don't think the flow pathways are going to make that big of a change, because the infiltration rates are going to be the same, the same volume of water is going to be the same, because the surface processes are very, very much fixed over the last 10 million years, probably, in terms of the structure of the site.

NELSON: Nelson, Board.

Do you think that it's possible to identify which paths are conductive?

FLINT: In a general way, you can identify which ones are conductive.

NELSON: At tunnel elevation.

FLINT: Well, I'm not sure you can, because when we, from at least my perspective, when we start to get to the tunnel, we're starting to look at a very uniform part of the site. We don't have these high exchanges that we see when we look at a different part of the site. I don't know if I have a map of infiltration that comes later, before this or after this.

So, you're looking at across the tunnel, you're looking at a more uniform part of the site, where we don't have that many major changes, although we do have some. We do go from the low area here, to a high area here. And, if you remember, this area in that one diagram under wash today, was some of the driest place we saw in the cross-drift. And, we put these instruments in right as the tunnel boring machine went through. Yet, they're really dry today, yet they might have more of the calcite as we go around this bend, because under past climate conditions, those are probably where the major pathways developed. And, under the future conditions, those are probably where major pathways developed. In our work on these climate change scenarios, we see these washes pick up a major amount of water.

So, if you want to say where the major pathways, where it's really wet, underneath there somewhere, where it's really dry, not under there. So, we see this contrast. So, that's the kind of way I can point at this in terms of current climate versus past climate, and where the channels are. But, beyond that, I can't do it from this particular approach in finding those pathways.

PARIZEK: Dan Bullen?

BULLEN: Bullen, Board.

Could you quickly go to the current conceptual model for flow in the unsaturated zone? That one.

Actually, I was interested in sort of your opinion with respect to where we are in the repository horizon in the welded tuff unit, specifically in light of a couple comments you made. And maybe I didn't get these comments right. But, you talked about the fact that in the El Nino years, we had a lot of infiltration, and then we had the repository sort of draining, and the draining rate was kind of on the order of 20 millimeters per year?

FLINT: That wasn't the repository.

BULLEN: That's at the surface?

FLINT: That's in the near surface. That's the top 6 or

7 meters.

BULLEN: Well, then, let me ask another follow-up question. The observation you made was that there's not much lateral diversion in the regions except for maybe the Paintbrush; is that right?

FLINT: There is not much lateral diversion. We calculate there's not much lateral diversion, we calculated maybe up to the 200 meters, but for the most part, we think it's lower than that. Where I think lateral diversion might be possible is part of the matrix flow phenomenon, where if you have a high infiltration rate over the PTn and a lower infiltration rate, you're just going to have a wetter PTn and a drier, and so you're going to want to have movement of water toward the drier. But, that's a matrix flow, not a capillary barrier effect.

BULLEN: Okay.

FLINT: For the most part, over the repository, no, I don't think there's enough of a capillary barrier to cause lateral flow. So, I think what we see in terms of the near surface on the order of, and this is a question I think Bo might have to address, too, on the order of 6 or 7 millimeters a year flux that may be going through the Topopah Spring. We only have about 6 or 7 millimeters of flux in infiltration above the repository horizon. So, it's hard to say. Maybe it is, you know, 20, 30, 40 per cent is what their models calculate, and maybe our calculations are correct, it's about, you know, less than 5 per cent or more. The higher the flux, the less lateral diversion.

BULLEN: Bullen, Board.

Then a follow-on question is if I put a heat generating source in there in the tunnels, and I'm starting to move water, will I have the necessary lateral diversion for it to shed between pillars, or will it just go up and come back down?

FLINT: That's a question I don't think I'm going to be able to answer. It's not a capillary barrier, because above it, unless you're getting above the PTn, then--and, I don't think that's the case, so I think you're still dealing with flow in the fractured system in the Topopah Spring, and you're not dealing with the contrast between the Tiva and the PTn, which is what causes our capillary barrier.

BULLEN: So, in your estimate, the model that we have for shedding between cooler pillars is still accurate?

FLINT: I don't have any reason to say it's not. But, I'm not a good person to ask that question to.

BULLEN: Thank you.

PARIZEK: Rien?

VAN GENUCHTEN: I have quite a few questions. I'm not sure where to start. But, one thing I'm still concerned about, and you raised it several times, is the PTn. Past conceptualizations suggest a lot of lateral flow. Now you don't. And, you say when you improve the numerical scheme and you build in more layers, and so on, you get less and less flow. I do understand, though, that--or less lateral flow. You still have some preferential flow mechanisms that can generate preferential flow there in the PTn; right?

FLINT: Yes, you do.

VAN GENUCHTEN: They're also, in our thinking, and in your paper, you mentioned that there are still a couple of fractures, or heterogeneities that can cause preferential flow.

FLINT: Yes. Faults can cause--certainly faults can do that, and then there are probably other features. The PTn is not uniform. As we go further to the north, the Yucca Mountain member becomes welded in the PTn, I think moderately welded. And, so, the PTn actually changes from north to south, so things are quite a bit different in the north than they are the south.

VAN GENUCHTEN: So, you still do see, in your mind, or your view of things, still, that there is, even though it may make the flow process much more uniform, that there's still quite a lot of mechanisms there that can general preferential flow from the PTn into the Topopah.

FLINT: Okay, there's two things here. One is the major mechanism I think that causes preferential flow through the

PTn is the faults themselves. I think we have more uniform flow through the PTn, the rest of the places, but what causes the transfer of water from the PTn to the Topopah may be a lot related to--if you could strip off everything above what the Topopah Spring looked like prior to the deposition of the first layers of the PTn, where we have this welded vitric cap lock, you probably are going to see a lot of these cooling areas, little deposits, depositions, highly fractured zones, we saw them in the north ramp of the cross-drift. I think it's been postulated that there are quite a few of these. So, it's sort of more of an undulating surface with all these broken zones as they cooled quickly, and then that was deposited over.

Now, these are probably going to break up a lot of the flow. This is an issue that maybe the geologists can address more, but that's our understanding, is that these features of the interface between the Topopah Spring and the PTn, between the welded and non-welded, has a lot of these heterogeneities that even though if you have a uniform PTn, it's going to be those zones that are going to allow the water to come in, and it's going to be those zones and some small faulting that are going to be what stop lateral diversion for the most part.

Even our idealized situation, we get this lateral diversion, we don't have all the micro-structure in the system, we don't have the small faulting in the system, we don't have all of that that's going to really keep lateral diversion. I mean, we have a hard time getting lateral diversion in engineered barriers, let along in natural systems.

VAN GENUCHTEN: So, in the earlier models, did they have the lateral flow in the PTn go over to the large hole there.

FLINT: In Scott's early model in 1983, they did not think there was a lot of lateral diversion. They thought most of the flux went through the PTn into the Topopah. In the model DOE and Montazar and Wilson's model, they thought the water would go across the top, I think they said about 4 1/2 millimeters of infiltration, 4 millimeters would go horizontally and down the faults themselves, and that's where the flux would go, and very little through the PTn. But, we've seen how wet the PTn is. I mean, it's almost a tenth of all our water potential in parts of the PTn. It's a fairly wet place.

VAN GENUCHTEN: Can you go to your Figure 8 in your paper, that review paper.

FLINT: The recharge paper or the hydrology paper?

VAN GENUCHTEN: Reviews of Geophysics.

FLINT: Hydrology of Yucca Mountain. Which figure?

VAN GENUCHTEN: Figure 8. That's where you had these chlorine 36 correlations mostly with--correlations with mostly the faults.

FLINT: Right.

VAN GENUCHTEN: Do those things go through the PTn then also?

FLINT: Yes, they do.

VAN GENUCHTEN: Including these lateral barriers, and generally preferential flows right here?

FLINT: These go through the PTn in all locations. One of the faults actually is a very steep dipping fault, and it goes through the PTn at quite a bit different location than the near surface. But, it was under where it went through the PTn that we found the bomb pulse isotopes, which gave us more faith in the model that it was the fracturing of the PTn that allows the fast pathways to get through. We couldn't understand why we had bomb pulse isotope in an area that didn't have a fault until we found the fault above it crossing the PTn above it, and going off at a sharper angle.

VAN GENUCHTEN: When I saw this figure, I was quite focused on these few points that are not associated with a fault. Has there been any work done to maybe say that this is not just happens to be a set of continuous fractures, but maybe it's a larger structural unit?

FLINT: It could be a different unit. It could be another feature that we don't see. It could be a buried fault or a hidden fault within the PTn. It could be a fast pathway within the PTn, fingering of some kind that we haven't identified as a mechanism yet, and I don't know what those particular ones happen to be. But they could be some feature I would guess having to do with the PTn.

VAN GENUCHTEN: One thing I was wondering about is in your, and again, I look at this review paper, and in here also you mentioned net infiltration, and you say percolation, and then you recharge. Do you consider those in the end to be equal?

FLINT: Yes. And, I made the one exception, and this is a paper that's coming out in A.G. Monograph in a couple of months where we talked about these mechanisms and trying to better define the mechanisms, is that net infiltration will become recharge, with the exception of some possible vapor flow taken back to the surface, which Ed Weeks has worked on, unless you intersect a perched water system and that water is discharged through a spring rather than into the regional aquifer. And, that's the point at which net infiltration will not become recharge, unless you consider recharge going into that perched water body, which some people could do. But, a lot of the springs that we see in the desert system are perched systems that are above the regional aquifer, and that net infiltration does not become recharge, but becomes discharge.

VAN GENUCHTEN: You mentioned it yourself, I still, in thinking back on some of the talks of Ed Weeks that I heard, is this vapor phase component that makes your percolation or your recharge rate less than the net infiltration rate, is that considered to be important?

FLINT: It's not considered to be important. Well, I've talked to Ed about this many times. He would struggle to get a half a millimeter a year loss of net infiltration through this mechanism, and he said it's probably an order of magnitude lower than that. So, if we're looking at 5, 6, 10 millimeters a year, and maybe a tenth of a millimeter, .05 millimeters in this vapor flow, it's going to be an insignificant mechanism.

VAN GENUCHTEN: Okay.

FLINT: That's Ed's thought. And, Ed Kwicklis's analysis. Ed Kwicklis did a flow analysis and found the same thing with a numerical model.

PARIZEK: Frank?

SCHWARTZ: Yes, Schwartz. I had two questions.

The first question is I'm still not exactly clear, kind of confused, as about the physics that's involved in accommodating the relatively high flux through the sort of matrix part of the system. I mean, do you--you have the issue of potentially keeping the matrix not saturated, but under saturated, yet at the same time, provide fairly high flows through that system. Now, what is the sort of conceptualization that lets that all happen.

FLINT: I hope Jim will address it a little in his talk, too, because it's an important component. First of all, we think in terms of matrix saturation. We're looking at, just so in the Topopah Spring, we're looking at about 90 per cent saturation. It's only a 10 or 11 per cent porosity rock, so it's still fairly wet. Measurements that we have suggest under the higher infiltration rates that it may be eight/tenths of a bar. And, all the fractures that we've looked at would have lower permeabilities at eight/tenths of a bar, so if you're going to have fracture flow as the fast pathway evidence, as our fluxes from the thermal analysis suggest, of our fluxes from the chloride 36 analysis, and the chloride say we should have this 5 to 8 millimeters a year, we can't carry it through the matrix. The matrix isn't wet enough to be an equilibrium with a hypothetical fracture.

Then, we have to have a decoupled flow between the fracture and the matrix, coupled in that it's going from the PTn into the Topopah Spring. Then, it's flowing through, I think Cliff Ho suggested 2 orders to 4 orders of magnitude decoupling, so instead of one to one, it was .0001 connection between the fracture, the flowing fracture itself, and the matrix, so that you wouldn't get the equilibrium.

And, the work that we tried to do at this ring analysis is we showed that you could actually get back to a 2

1/2 micron fracture, and come back into equilibrium. So, if you're flowing through that size fracture, in some areas, you could have that relationship exist. But, it has to be a decoupled system in which the fracture and the matrix are not talking to each other.

When we look at the geochemistry of the water, we might find that they are different, except in the perched water bodies, then the chemistry in the perched water and the matrix seem to be more similar, because they have the long interaction time. It's really, the whole idea is you have to have a decoupled fingering, is one way they look at it.

SCHWARTZ: That I was going to ask you. I mean, is fingering one example that brings about decoupling?

FLINT: Right. Right.

SCHWARTZ: In other words, you're just going through a small part of this area.

FLINT: Yes, you're just going through a small--right, exactly. Rivulet flow is another way to look at it. Fingering is one way to look at it. But, a very small part of the fracture is flowing.

SCHWARTZ: Okay.

FLINT: Less than a per cent.

SCHWARTZ: I had one more question. The question I had was your conceptualization talked mainly about sort of matrix issues, and the big fault issues. Could you talk about what you think the scales of fracturing at a smaller scale, and how that scale development may influence the kind of pattern you see. You've probably looked at more of that than anyone, of sort of scales of fracture development at a smaller scale may be important, as well.

FLINT: I didn't bring it out here, but when we started looking at water potentials in the unsaturated zone, we found a very strong correlation as we went through the middle non-lithophysal where you have lithophysal and non-lithophysal zones, and the change in water potential changed very noticeably within these zones. So, the fracture system is in contact with the matrix, the more fractures, the wetter the rock seems to be. The less fractures, the drier the rock seems to be.

But, when I look at this system, I think of it in terms of a, if I was a really, really giant person looking at this, it looks like porous media in a sense because of the way the fractures are, ubiquitous through a lot of the Topopah Spring, through these different layers, and that the infiltration rates I think are high enough that all these fractures may be playing a role. But, we do see this relationship between water potential and the fracture density. But, we're seeing more detailed, smaller fractures as we look at more detailed studies, and a lot of our work in the ESF early on started with only the really big fractures.

But, the work we did with these small parameters suggested that maybe the small fractures, the ones we don't map at all, that we don't have much record of, are what may be carrying the flux at the same water potentials as the matrix.

But, these are just a new area that I've been just working on with David for a year or two trying to just sort through this.

SCHWARTZ: Thank you.

PARIZEK: Rien?

VAN GENUCHTEN: I'm sure we'll revisit some of these issues, matrix fracture interactions, this afternoon; right?

PARIZEK: Well, you might want to get him before he gets away, because we can't guarantee he'll be here this afternoon.

VAN GENUCHTEN: One question I'm always interested in is this, it connects with the earlier talk about coatings. Does the effect of hydraulic conductivity across the matrix affect the interfaces? And, as you know, there were some studies with small rock samples that was also in the NRC book, where they showed that the conductivity saturated can be decreased by up to 6, 7, orders of magnitude. Is that still being looked at? Is this also an explanation for this lack of interaction between fracture and matrix? You know, which goes back to the active fracture?

FLINT: Yes, the idea that the water could be flowing in the fractures completely, but that only 1 per cent of the matrix can take in water, because of a change in hydraulic conductivity due to fracture coatings. We know that in the near surface, certainly, in the near surface in the Tiva Canyon, we see fracture in-fillings, we see fracture coatings, and we have taken those into the laboratory, made measurements like these on the paper in this particular book, and showed that the hydraulic conductivity of the rock is altered in and around these fractures, and can be easily altered in and around these fractures by this near surface weathering.

I have not looked in depth at the deeper units and looked at imbibition rates in the mountain. We did a paper we published a couple years--several years back now, on imbibition rates in G-tunnel and trying to look at the fracture in-fillings, and those didn't seem to be bothered at all by the fracture coatings. It seemed to be more uniform, and went deep into the rock when we flooded the boreholes.

So, the only experiment that I have didn't suggest that the matrix had these real preferential, high in permeability, low permeability areas, because of coatings.

VAN GENUCHTEN: These coatings would be especially prevalent where the flow paths are.

FLINT: Right.

VAN GENUCHTEN: And, that's what I understand from the earlier talk. And, so, how do we know when you take these samples and bringing in and doing your centrifuge methods, whatever it is, that those are from the areas where you have these preferential flow paths?

FLINT: The measurements of what, now? Are you talking about permeability of the rock itself?

VAN GENUCHTEN: Yes.

FLINT: Because we're not looking at--we did some matrix imbibition experiments on rocks, and we did show that the armoring of the rocks due to weathering or due to decomposition, the weathering at the surface where the fracture was exposed to air flow, those did have a low permeability, without question. We showed that very, very clearly. Deep down where they don't have the coatings, I'm not sure, where they do have coatings, my guess would be yes, they would be. But, they talked about a lot of the coatings that they're talking about, a lot of them are occurring in these lithophysal cavities. And a lot of the smaller fractures, where they don't see coatings may not have this problem at all. They may not have any coatings. I don't think overall that you're going to be able to do that. I think it's still going to have to be a decoupled fracture/matrix model that's going to make this work. But, I probably will be here this afternoon.

PARIZEK: Parizek, Board.

That present illustration still leaves the elevated chlorine values in there. But, we're really in a state of flux in that regard, are we not, in terms of just trying to validate the presence of elevated chlorine? I mean, suppose all of the points above the shaded horizontal zone there disappeared because you couldn't justify them.

FLINT: I'd have to put up the tritium graph then, the bomb pulse tritium.

PARIZEK: Yes. So, it wouldn't change. Your conclusions would still be similar?

FLINT: Well, I mean, you know, the tritium data, the technetium, the chlorine would be very similar. From a practical standpoint, I don't see why you would have a feature that goes all the way through from the surface of Yucca Mountain to the Topopah Spring that breaks up the PTn, and we've been through some of those faults and looked at them, that you wouldn't be able to carry flow through those over 50 years. So, my conclusion would be the same.

PARIZEK: The PTn has an umbrella on this, or tin roof, was always a kind of pleasant thought. But, if I was to do the shaft, or say for confirmation testing, a shaft down into that zone, and if I actually had perched water during pluvials, would I not have secondary minerals that were on top of fractures within the voids, growing in, so that from time to time, it actually was 100 per cent saturated?

FLINT: Oh, we do show the top of the PTn as having been 100 per cent saturated.

PARIZEK: But not necessarily serving as lateral flow?

FLINT: It could have served as lateral flow. It doesn't today into the flux rates. There's some lateral flow, certainly. We do see alteration. Dave Aneman and Lori did a lot of work on alternation of mineral zeolites. There's zeolites in the top of the PTn because of the high saturations there. So, there's been a lot of weathering. Whether that high saturation, I shouldn't go so far as to say that's going to cause lateral flow, because the transition is so gradual across there, so we may not see that. And, I don't know if we have evidence for that at the top of the PTn certainly. But, under weather conditions, remember now, the higher the infiltration rates, the less lateral diversion you're going to get as a percentage of the flux. So, the higher rates cause us to have less lateral diversion. The low rates, we get more.

PARIZEK: Any other questions? We have two members of the public that would like to ask questions. Maybe if we restrict their time to just a couple minutes each, we have Jacob Paz. Yes, we do thank you very much for staying, and maybe you will be here this afternoon, but we appreciate the chance for the questions. We'll let you off.

If you could keep your remarks brief?

PAZ: I'll be very short. Number one, I received a letter from the Environmental Protection Agency, and I'd like to thank the Board for suggesting that I communicate.

Generally, the letter states the following. That I suggested the EPA should take a second look for its standards for the Yucca Mountain repository in light of recent research. I understand that your concern that Yucca Mountain standards should be based on up-to-date scientific information.

Abbreviated, that the EPA now is a co-sponsor with the NRC, National Research Council, and will review all the relevant data contained risks at the low dose, and publish recommendations within the next year. Once the NRC completes its study, it will review the radiation risk methodologies and make appropriate modifications as warranted.

I think this is significant. I wait to see how they're going to address it scientifically. Thank you.

PARIZEK: Thank you, Paz. Sally Devlin?

DEVLIN: Good morning, everybody. And, as usual, I want to welcome everybody to Nevada. Thank you so much for coming. I hope we'll be hearing that your meetings in the future will be in Pahrump. But, I did have something to say, and I want to say thank you, I see Russ is here, but John Arthur and Madam Chu are not here, and I did want to thank them for the six KTI books that they gave me. And, at the last meeting I gave you my report on the first three. I have not completed the other three. They're a lot harder, and all I can say is that I'm still reading the in drift chemical environment and the waste package designs.

And, I do have to let everybody know that I don't understand the exchangeable terminology for coupons for specimens. Russ, where did you come up with the coupon word that's in your report? And, it seems that there hasn't been a test of any of this stuff, I'm talking about the Alloy-22 and the drip shield titanium, which goes from 1 to 24, that has been tested for more than five years. And, it was suggested that since I was here, and my friends in Pahrump said why haven't they actually dug the hole and done a prototype and really done some science.

So, as far as I am concerned, and this is my personal point of view, that the prototypes and the lynch pin, and so forth, have not been done, and here next year, you're going to licensing. So, I don't think that's very nice.

The other thing is on the menu today, and that is when we talk about hydrology, to me, the most important thing, and again, with the alluvial fans and all that that the DOE is praying for a lot of clay. Well, I'm sorry, but you are not doing a proper job with my colloids or my bugs. And, MIC, you are ignoring. It is mentioned, it is not explored, and I don't see how you can do licensing without it.

And, the one thing I learned, and you know I know nothing, I go to people who are metallurgists and engineers and all kinds of stuff, and that is if you have the titanium drip shield and you make it with some palladium in it, and then you have the coupon of the Alloy-22, which emits hydrogen, you're going to have a big boom. And, I don't know if magnesium chloride has anything to do with that, too. But, it really disturbs me because you are not doing in situ. You have no prototypes, and so on. And, I think after eleven years, that you should have.

But, anyway, I do want to talk about, and Dr. Flint, who I just love, because I love all those USGS guys, and he says about the fractures and the fissures and the ponds, and so on, and I know that that is Yucca Mountain. And as Jacob told you, you know, we're going to go to that meeting on Monday with Senator Reid to find out about the terrible stuff from the silicosis, and what have you, that can be present in the five miles of rock that are sitting out there.

This is terribly important because I really don't feel that you consider, and in that letter from EPA, we the people that are being investigated for these problems are called bystanders. So, now we've got coupons and we've got bystanders, and I've never been called a bystander in my life. And, if anybody thinks I'm going to stand by, they're crazy.

But, I do want to get back to one of my reports from the last time, and that is it hasn't been mentioned, and it should be mentioned, and that's my volcano, my Ingrid Bergman. Does everybody remember Ingrid? And Ingrid is only 12 miles, 25 kilometers, from where the proposed repository is to be placed, in that 18 kilometers, or whatever it is. And, if Ingrid does blow, and the repositories are there, of course the world will be destroyed as we know it, except for the DOE, and they will all live. And, when they decided that the ash cannot go to Beatty, cannot go to Death Valley, cannot go to Pahrump, and they put this in writing, that in 35 years when the DOE repopulates Amargosa, that this is what it's going to look like.

PARIZEK: Let the record show that Sally is showing two posters at this time, which is not on audio.

(Sally Devlin's poster says, "When Ingrid Bergman the volcano erupts and both repositories are destroyed, as well as the whole world's population, except for the DOE, they will repopulate Amargosa with.." and there is a picture of a volcano and a two headed man.)

PARIZEK: Sally, are we done?

DEVLIN: I'm done.

PARIZEK: Okay, thank you very much.

Now, we're about ten minutes later than what we were going to be, so for lunch, let's be back here no later than 1:25. Let's say 1:20, because I guess my time is two minutes too fast.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

CERLING: Good afternoon. We're going to start the afternoon session now. We're running a little bit late, so we'd better get going.

Welcome back to this meeting this afternoon on the Nuclear Waste Technical Review Panel on the Natural System. I'm Thure Cerling, and a Panel member. This afternoon, we'll continue with the theme of the Unsaturated Zone Fluid Flow and Radionuclide Transport.

This morning, we presented a list of questions that outlined the central purpose for this meeting, and the talks will continue to address those aspects of those questions.

The first talk of the afternoon will be presented by Bill Murphy at California State University, Chico, and he'll talk about the role that secondary minerals play in the transport of radionuclides from the natural (inaudible) and deposit in Chihuahua, Mexico known as Pena Blanca.

I'm just making sure I've got everything right here, and in the right direction. The Pena Blanca analog site is being used by DOE and Ardyth Simmons of Los Alamos will make a presentation following Bill Murphy.

There's a slight substitution in the schedule, and Russ Dyer will give a short presentation before James Houseworth, and James Houseworth will then speak on DOE's conceptual models and independent lines of evidence from models in the unsaturated zone.

Then, we'll take a break, and we'll follow on, and George Moridis from the Berkeley Lab will discuss the transport processes, absorption, matrix diffusion and colloid facilitative transport, and how they're represented in DOE models. And, then, finally, Bruce Robinson from Los Alamos will discuss modeling predictions for the transport of radionuclides through the unsaturated zone, and how those predictions are abstracted for the total system performance assessment, also known as TSPA.

After that, we'll have a public comment period, and if you wish to speak at that time, make sure you see and sign up with Linda or Alvina in the back. We'll attempt, as always, to accommodate all who wish to speak, but we may have to limit the time, depending on the number of people who wish to speak. And, as always, we welcome written testimony for the record.

And, last of all, please shut off your cell phones, or we'll get some other sort of call from our AV people. And, with these preliminaries out of the way, it's my pleasure to introduce the first speaker, Bill Murphy. Bill, take it away.

MURPHY: Thank you very much.

I would like to thank the TRB for this invitation. It's my pleasure to contribute some of my ideas and also to share work that was largely, almost exclusively, conducted on behalf of the Center for Nuclear Waste Regulatory Analysis. But, I must note that I'm not representing the CNWRA at this meeting. I'm representing myself at the invitation of the TRB. There are other Center employees here who can represent the Center. But, nevertheless, much of the work, or almost all of the work, I'll talk about today was conducted by the CNWRA, and with their support. And, I need to acknowledge that contribution and the contribution of my many colleagues there, and friends, you'll see their names scattered around this information.

I'm going to speak primarily about Pena Blanca and also about those aspects of studies at Pena Blanca that seem most important to me, with regard to the performance of the proposed repository at Yucca Mountain.

These are organized by a set of key observations. The first set of observations regard secondary minerals, and secondary minerals are an important part of the system at Pena Blanca, particularly secondary oxidized hydrated uranium minerals. And, I think it's widely accepted, at least I firmly believe, that radionuclide releases at Yucca Mountain will be controlled in large part, not exclusively, but in large part by the properties of secondary phases after spent fuel, which is dynamically unstable in that oxidizing hydrated environment, comes in contact with the environmental conditions.

And, through the years, there has been, in my view, a favorable convergence of information from theoretical studies, thermodynamics and kinetic studies, experimental, laboratory studies, and natural analog studies, in particular, from Pena Blanca, a converging set of evidence for the role of these secondary uranyl, that's oxidized uranium minerals, in controlling radionuclide releases.

Here is a picture of the adit at the 0 meter level at the Nopal I ore deposit in the Pena Blanca district, and here we see highly brachiated silica tuffs. There are many remarkable similarities between this site and Yucca Mountain, the chemistry of the rocks, the relatively arid climate, the unsaturated hydrologic conditions. The big difference, of course, is that there's a big uranium deposit at this site. The genesis of the deposit was under reducing conditions, and the primary ore mineral was uraninite, and that uraninite has been almost entirely oxidized, and the rate of that oxidation is clearly rapid, or was clearly rapid relative to the removal of uranium from the system, because much, or most, of the uranium is still there in the form of secondary uranyl minerals.

There are remarkable similarities between the Nopal I site and Yucca Mountain, and there is fantastic access to the site. It's exposed right at the ground surface. It was mined for uranium for a while, but then the mining was abandoned, leaving it available for study. It's a remarkable site in the context of Yucca Mountain studies.

There are also important differences between the sites that have always to be kept in mind in interpreting data from the site. There are sulfite minerals that are not typical. Yucca Mountain, there is silicification of the ore zone. We don't know precisely the temperature conditions, formation, or for that matter, the temperature or saturation conditions for the alteration or the uraninite and the formation of the secondary phases.

Nevertheless, it provides a very special case for study of properties and systems like Yucca Mountain on time scales, in particular, that are long relative to any accessible in laboratory studies.

This is a picture of a thin section. It's just a photograph. It shows one of the remarkable features of the site. On the right side of this diagram, there is uraninite, along with silica in the black portion of this rock. This is a very silicified portion of the rock, the sort of brownish area is highly silicified. It's this silicification that's protected some uraninite from oxidization at the site, I believe, limiting access of oxidants and water.

So, we see preserved at the site an entire suite of mineralogy, from primary uraninite, which has the same structure and largely the same composition as spent nuclear fuel. It's about 5 per cent other components, other than uranium dioxide, like spent fuel is, the components aren't the same, but it's unlike other analog sites, uranium deposits that are very old and dominated by decay products like lead, of uranium. This is a young deposit. The ore deposit itself is about 8 million years old, by our rough chemical uranium-lead data. And, so, it's not dominated by decay products.

There is a whole suite of secondary uranium minerals which I'll describe in some detail in a moment. There's the yellow materials in this figure, and it's hosted by a silicified tuff where the ore occurs. There's paolanite alteration of feldspars in this area. So, the rock has been altered in the vicinity of the ore deposit, and there's quite an abundance of secondary uranium minerals.

Here's one more picture that shows weeksite, which is a potassium uranyl silicate hydrate mineral, the pretty acicular crystals are this uranium mineral forming in fractures close to the vicinity of the primary uraninite deposit. And, obviously, here precipitated in a fracture.

The matrix is mostly feldspars and quartz.

This is a slide that illustrates part of this convergence of ideas, and it's one that's been well recognized by the Project. The column on the left shows mineralogy at Nopal, and the column on the right shows mineralogy in very long-term experiments. These are experiments that were a decade long, or so, that were designed to mimic Yucca Mountain conditions. They were J-13 type water was dripped onto synthetic uranium dioxide, and secondary minerals formed.

And, the sequence of secondary mineralization in the two sets of conditions, with widely differing time scales, were very similar. First, uranyl oxide hydrates, and then uranyl silicates, and this converging pattern of secondary mineral paragenesis in a way bounds conditions that we could expect potentially to happen at Yucca Mountain.

It's important always to recognize there are differences between the systems. There's a general progression in both of these sets of data of increasing incorporation of environmental components in the secondary phases, first just uranyl hydrates, and then silica gets involved, and then the alkaline earths and the alkaline metals get involved. That shows up in the experiment. I think that could also be a consequence potentially of pro-grade alteration, changing temperature conditions, in the case of Nopal. There are still lots of uncertainties with regard to the timing and the conditions precisely of the alteration at Nopal, and I'm pleased that work is being conducted at this site still.

So, the timing is of great interest here. We have uraninite that may be 8 million years old. We have uranium-lead data on uranophane at about 3 million years. We have young secondary phases that are the latest forming materials at the site. And, the latest forming materials are the ones most relevant to the time scale of the repository. We have opal and calcite that are both rich in uranium, and they've been dated at about 50,000 years. There are a number of dates that suggest some kind of mineralization event at 50,000 years. There's data from the DOE Los Alamos suggesting that some of the iron oxyhydroxide alteration phases are older than can be dated by uranium decay series analyses.

But, we have a geologic time scale here, short, as geologic time scales go, but it's certainly long relative to even extremely long experiments. Here's the time scale of the Argonne experiments, and the bars show the timing of the formation of these various secondary phases.

The second key observation has to do with alternate performance assessment models. We have found, indeed, that if we can take account of the role of secondary minerals in performance assessment, at least there's the potential to showing that the predicted performance is improved.

And, we've tested a couple different scenarios that explore data from Nopal and Pena Blanca. The first was an estimate of dissolution rate of fuel in performance assessment models, based on a limit on the oxidation rate of uraninite at Nopal. Obviously, the oxidation rate places a limit on releases from spent fuel. So, we've made a maximum estimate of the oxidation rate of uraninite at Pena Blanca using the 3 million year date for the uranophane, and large conservative estimates of how much uranium has actually been removed from the system by water leaching through the system. And, we've introduced that in a performance assessment model as an alternative for the source term, for the reaction rate of uraninite.

We've also considered an alternative performance assessment model in which we considered the coprecipitation of radionuclides in secondary phases. In the model, we used schoepite, which is uranyl hydrate, as a secondary phase of concern. In the absence of good data for the distribution of trace elements between, or especially actinite and fission products, between aqueous solutions and secondary uranyl minerals, we just guessed that the ratios would be the same as they are in spent fuel as a matter for comparison, and assumed that as schoepite grows as a product of alteration of uraninite, it also includes those radionuclides that are in the matrix of spent fuel in its structure. And, then, subsequently, those species are released as controlled by the solubility of schoepite in the waters that flow by.

So, this is a CCBF showing these performance models, and we see improved, but comparable performance modelled or estimated in these calculations, considering this curve represents the schoepite model, in which the radionuclides are included in schoepite. This curve shows the Nopal oxidation rate limit. And, for comparison, this is uraninite or spent fuel dissolution rate, interpreted from PNL data by the NRC and the CNWRA, and this was the dissolution rate estimated from experimental studies in one of the DOE performance assessments.

So, we see some improvement in performance by considering these alternate models that aren't better or worse, but a useful comparison, in my mind. I think that given the recognition that secondary uranium minerals will play a role in the alternate releases from Yucca Mountain, it's reasonable to consider them in performance assessments. And, that's what we attempted to do here.

I mentioned coprecipitation. This is the incorporation of actinite and fission products in secondary products. This has been widely discussed, and a to a certain degree, it's been studied experimentally. There is still a lot of work to be done for this problem to be judged very quantitatively, in my opinion. We just guessed at numbers for our distribution coefficients in our studies. There have been conflicting results from--not conflicting, but differing interpretations of results in spent fuel dissolution studies, in which, in particular, neptunium has been looked for in secondary phases, that one set of studies by one spectroscopic technique showed perhaps ten times more neptunium in the schoepite than there was in the spent fuel relative to uranium.

And, then in the last year, there's been another technique applied to studying the same kinds of phenomenon, and found very much less than that. And they went and re-interpreted the original interpretations. I think there is still a great deal of uncertainty. There have been studies that have been analyzed by Eugene Chen, in particular, for the Yucca Mountain project, in which he's looked at relative releases of uranium and neptunium, and coincidentally, I think, concludes that the distribution coefficient is about the same as we guessed, a distribution coefficient of one based on data for releases. But, the data themselves are rather scattered, and the experiments that those good ideas were extracted from weren't really designed to measure the phenomenon that's been extracted from them.

So, I think that equilibrium solubilities and distribution coefficients are quite uncertain from both a thermodynamic and a kinetic perspective. There are good data in the geochemistry literature that shows that the actual coprecipitation in calcite and in some other phases is a very strong function of how fast the minerals precipitate. And, there's a very strong potential gradient, chemical potential gradient, driving spent fuel oxidation in an oxidizing environment, and there's certainly the possibility of kinetically controlled growth of these secondary phases, and the actual distribution of actinides and fission products in secondary uranyl minerals may well be controlled by kinetics as much or more than by thermodynamic relation. So, this is a great subject for more work, in my opinion.

The next observation regards radioisotope constraints and effects, and there are really two topics that I will talk about here. One is the use of uranium and thorium decay series isotopes from Nopal to place temporal constraints on migration of these radionuclides.

And, the second is the observation from Nopal and elsewhere that the daughters of alpha decay tend to be preferentially released in water/rock interactions. And, there are potential performance consequences of this notion that to this time, have been largely neglected, or nearly completely neglected, in performance assessments. I'll address that in a moment.

So, here are data from Nopal. This is the uranium 234 activity over the uranium 238 activity. These are radioactivity ratios, not concentrations. For a system that's closed for a time period that's long relative to the half lives of the daughters, this ratio goes to one.

We see values that DBA from one at Nopal suggesting that the system has been open on time scales relative to the half life of these species. And, particularly in the waters, perched water and seep water from Nopal, and here, we see elevated U-234, U-238 ratios. This is a consequence of the preferential release of alpha daughters. U-234 is like the great grand daughter of U-238, and U-238 decays by alpha decay.

So, the reason that U-234 is elevated in natural water is because it finds itself in damaged sites due to alpha K, or in cases actually ejected into solution. And, so, we see evidence here, it's somewhat a function of the concentration of the uranium in the rocks, or in the water, and this preferential release phenomenon would probably be more important under reducing conditions where solubilities of uranium are very low.

Here are some more uranium decay series data. These are all data from the Nopal I site, and they predominantly are fracture filled materials. And, so, to the extent that the fracture fill materials show the values of these activity ratios that differ from unity, indicates that the system has been open on a time scale that can be computed based on the half lives of the species. There are data here that have fairly large uncertainties. Some of them reside in this zone that's called the multi-stage history zone in this figure. David Pickett, who is the principal author on this work, has interpreted these data to indicate that there's been mobilization of uranium, and then re-mobilization. We have a complex history of mobilization and re-mobilization of uranium at the site, as indicated by these data.

There are also some data on this slide from Los Alamos using much more precise analytical techniques. They tend to fall on this line of equal activities of Thorium 230 to Uranium 234. In contrast to the CNWRA data, this may be a consequence of a variety of things, or a combination of things. I don't know why this discrepancy exists precisely. The Los Alamos samples were provided to them by the Center for Nuclear Waste Regulatory Analyses. So, in some cases, they were actual splits of the same materials. And, in many cases, there's a close overlap between the data set, although there are none off this equal activity ratio line among the Los Alamos data.

We're concerned that this may be a reflection of uncertainties in the data, and haven't found any reason to believe that that's necessarily the case. I'll point to this figure that's not often cited or observed. It's published in a rather obscure place in Proceedings of the Seventh EC Natural Analogue Working Group Meeting from 1997. And, it shows this same thorium 230, uranium 234 activity ratio, and for fracture fill materials, in particular, there seems to be a systematic variation in that ratio with respect to distance from the boundary of the ore deposit, which indicates to me that there's a systematic deviation from unity in this ratio, and that maybe it does indicate open system conditions.

Now, I'm going to back up and use this constructively this time. My second point with regard to radionuclide release issues has to do with this preferential release of alpha decay products. This is widely recognized in natural systems. It would not be recognized in spent fuel dissolution studies, because it takes time for the alpha decay process to occur, and for the radionuclides to find themselves in the sites of the alpha decay. So, it's not something that would be observed in experimental studies, and it is observed in nature. And, to this point, it's not included in anybody's performance assessments explicitly, however, I invite you to a talk by David Pickett, my colleague, and me at the upcoming MRS meeting, where we'll show those calculations. I can't show them now because they are not published yet, and we're still working on it.

But, in any case, in an MRS paper a couple years ago, David and I published a table that illustrates that in the long term, a very large fraction of a number of important radionuclides will in fact reside in alpha decay sites. And, essentially, all the lead and radium 226, actinium, thorium, these daughters will almost exclusively, or exclusively, reside in alpha decay sites.

Some of the other important, potentially important ones include neptunium 237, which is a decay product of americium, and it, at its peak, 71 per cent of the neptunium 237 resides in alpha decay sites. And, so, we think there's a potential for preferential release of these species, and potentially a high effect on performance if this augmented release is taken into account. And, we're doing calculations to test that at present.

So, in summary, I think that secondary minerals will control releases of many radionuclides at Yucca Mountain. The alternative performance assessment models that have been generated taking their role into account show that taking them into account improves model repository performance.

Coprecipitation data presently are inconclusive. The data are sparse, and the data have not been fully developed. Thermodynamic and kinetic data would help certainly.

Radioisotopes at Pena Blanca demonstrate system openness at the site, and in particular, can be used to constrain the timing of system openness, which is very important.

And, finally, alpha daughters are released preferentially. This is widely recognized in natural systems, and we believe that performance consequences should be recognized as well.

Thank you.

CERLING: Thanks. And, we'll take some questions. Ron?

LATANISION: I'm wearing my geologist hat again. This is Latanision, Board.

I'm very interested in your slide that describes alternate PA models. And, your point here is that the dissolution of spent fuel based on estimates of oxidation rates--the dissolution rate of spent fuel based on a uraninite analog. These dissolution events are also what I would describe as structure property dependent, meaning that the micro-structure of the uraninite and the micro-structure of spent fuels must be similar enough that you can make some with some confidence that sort of statement. And, so, I'm wondering are the grain size, the phase distribution, all of the sort of characteristics of the petrography, I suppose, of the mineral and of the spent fuel, are they enough alike that you can feel confident with that?

MURPHY: They're not identical, of course, and I would not emphasize that they are. I pointed out the similarities. Spent fuel has a cubic structure like natural uraninite does. So, they have structural similarities. Spent fuel, of course, has been through a reactor and has a lot of radioactivity, and it's suffered damage in that regard.

Uraninite at Nopal has about 5 per cent impurities, which are different than the 5 per cent that occur in spent fuel. So, there are certainly chemical and physical differences between them. There are lots of other differences that would affect the oxidation rate as a limit on dissolution rates, hydrologic setting, the salification. Where uraninite is stabilized at Nopal, it's due to this more or less impermeable salicification that's encased it. That's a different condition. There are a lot of differences, and, so, I would not carry this too far. I think it's, recognizing those differences, it's remarkable that there's anything as close as there is.

LATANISION: Latanision, Board.

I was about to make the same comment. In fact, if we go two slides forward, I think you showed this is actually quite impressive, even on the same figure.

MURPHY: Absolutely.

LATANISION: I'm quite serious. I'm very impressed, and perhaps in a macro-scopic sense, they are similar enough that they do belong in the same ballpark. And, perhaps, as well, with the subtleties that we've just been talking about, phase distribution, volume fraction, et cetera. And, perhaps those two become much closer than they are now.

MURPHY: Maybe, but they are different systems, and I think we need to recognize that there are big uncertainties in the PA models based on dissolution experiments, as well as on the Nopal. You know, the uncertainties in these curves aren't confined to the alternative models.

LATANISION: Right. Thank you.

CERLING: Richard.

PARIZEK: Bill, if you'd look in the groundwater part of this system, do you think you can measure things in groundwater in quantities enough that would give you some idea of the rate at which things are leaching out of this mountain? Or is it maybe the flow field is contaminated with other sources, because there are other deposits in that area that raise a question, I know, talking about these same details.

MURPHY: We can certainly measure uranium and its decay series products in the unsaturated zone groundwaters at Nopal. We have such data, and I showed some of those uranium data. So, can we estimate the leaching rate based on those concentrations? Well, we'd have to quantify the flow through the system, which we can estimate, but isn't quantified particularly well right now. We've used the data to try to examine whether or not the system seems to be at equilibrium with uranium minerals. There are big uncertainties in the thermodynamic properties of the secondary uranyl minerals. So, I think there's the potential to gather a lot of relevant data at the site. And, one of the sources of uncertainty that we faced in our studies has been that all the samples were from the surface, from the ground surface, and, so, they weren't only affected by natural underground processes. They're part of a mined surface, and they were very close to the natural ground surface, even prior to mining. And, so, I'm very pleased that they're now core samples taken from depth, and I think those will be a step more realistic in their representation of what may happen at Yucca Mountain.

PARIZEK: Parizek, Board.

The impressive thing is that from the time of rock faulting and raising this up above the water table and allowing for corrosion, and so on, how many years have these deposits been exposed to weather and leaching, right at the grass roots level, for one hell of a long time?

MURPHY: The volcanic coast rocks are about 44 million years old, and the uraninite deposit itself, by our best measurement is about 8 million years old, and I'd be delighted to see more accurate estimate of that. The number we use as an estimate of the minimum time that the site has been exposed to oxidizing conditions is about 3 million years, based on uranium-lead dating of uranophane. It's been oxidizing at least 3 million years.

At one stage, we made some very gross estimates of uplift grades, and speculated on groundwater table and the height of the deposit above the groundwater table, and tried to estimate what a limit was to how long it's been in unsaturated conditions, and we came, I forget the exact number, it was some tens of thousands of years, as I recall.

PARIZEK: If you realize the water table is in the carbonate, and so I guess the lower body is elevated in tuffs, but on the other hand, leached down through there, you're going to run into unsaturated carbonate rock. Is that likely to cause some difficulties in how this would compare with Yucca Mountain?

MURPHY: I think that at this site, the tophaceous silicic rocks are deposited on top of cretatious limestones. And, are you referring to those carbonates?

PARIZEK: Yes, the water tables of the contacts.

MURPHY: Yes. My personal view is that the systems are almost completely disconnected. The unsaturated processes in the tophaceous rocks involving meteoric waters, and the present day inter-basinal aquifer that's probably primarily in carbonates, I think are separate systems, quite distinct from one another.

Now, in the geologic past when this site was below the water table, there may well have been circumstances of mixing. My personal view of the genesis of the ore deposit is one that involves mixing of waters derived from carbonates, reducing waters derived from carbonates, with oxidizing waters bearing uranium derived from tophaceous rock. So, I envisage their interactions in the geologic past, but the present circumstances I think the present conditions are very much disconnected. There's a little trickling of water through the Nopal site, and eventually into the carbonate aquifer system, but I don't think you can see it, its chemical signature. We haven't been able to in data we've seen.

And, particularly, the relevance in my view of Nopal and Pena Blanca is the latest effects, what's happened there in the most recent geologic time is the most relevant to what will happen in the next 10 or 100,000 years, or half a million years at Yucca Mountain.

CERLING: Frank Schwartz?

SCHWARTZ: Yes, Schwartz.

I had several questions. I enjoyed your presentation very much. The first question, at the analog site, what was it geochemically, what changed geochemically, actually triggered the precipitation of the secondary minerals?

MURPHY: Oxidation of primary uranium dioxide.

SCHWARTZ: Okay. The second question I had had to do with you talked about both an equilibrium and a kinetic model. And, what I was wondering is the reason you're interested in this kinetic formulation is an implied slower process to bring this about, or what is it about this kinetic model that makes it sort of different and special?

MURPHY: The secondary phases are to play a big role in sequestering actinides and fission products. Those actinides and fission products need to be incorporated in their structures, and there are fundamental thermodynamic relations that describe the distribution between neptunium and an aqueous phase and neptunium dissolved in a solid schoepite, for example. The data to support that are sparse, but one can formulate that relationship formally with thermodynamics.

What one finds, however, is that in effect, the effective distribution of trace elements between aqueous solutions and minerals can be very strongly a function of how fast the minerals grow. And, the faster they grow, the less fractionation occurs, whether the trace elements are excluded or included preferentially in the solid. And, so, in fact the degree to which actinides and fission products will be incorporate in shoepite or uraniphane at Yucca Mountain may depend as much on how fast those secondary phases form as to what the equilibrium distribution is.

SCHWARTZ: Okay. In your talk, you talked about Kd measurements. Are those sort of Kd's for the newly formed secondary mineral surfaces? Is that what the Kd's refer to, so you're looking at sort of a sorption kind of mechanism as a scavenging device as those secondary minerals are formed?

MURPHY: I'm not sure where I used--I used the value for Kd in the schoepite solubility model. Was that the context?

SCHWARTZ: Well, yeah.

MURPHY: It wasn't a sorption phenomenon. It was used as a distribution coefficient between a bulk phase and--a bulk solid and a bulk aqueous phase. It wasn't a surface phenomenon. It was just a distribution coefficient.

SCHWARTZ: I've got one question left, if I might.

The last question is how would you go about sort of developing more confidence experimentally or physically in the attenuation benefits that you might get through these processes you talked about?

MURPHY: That's a problem I've been working on for a long time, and one of my other colleagues, Jim Prikryl, at the CNWRA, and I will be presenting data on uranophane dissolution and solubility experiments that are being conducted at the CNWRA. I think that I'm gathering the basic thermodynamic data for these secondary phases first, evaluating the rates at which they grow, and eventually evaluating the equilibrium distribution coefficients of perhaps actinides and fission products or surrogates for those, and any of them, those are all legitimate potential experimental programs.

CERLING: Dave Diodato?

DIODATO: Diodato, Staff. Thanks for your talk, Bill.

I wanted to follow up on some questions Dr. Parizek raised, and then you responded to. You said, according to your estimates, this deposit is probably on the order of 8 million years old. And, then it had at least 3 million years of experience in oxidative type geochemical state, and then at least several tens of thousands of years in unsaturated hydrogeologic conditions. According to your best estimates, how much of the original mass of the original deposit is still present right in this immediate vicinity of the Nopal I deposit?

MURPHY: In calculating my Nopal oxidation date limit for the PA model, I did that calculation, and I don't have the number on the top of my head, but I'll look it up for you in papers I have with me. And, it was, I'll guess at my own hazard, I guess, it was something like 20 per cent has been, an upper limit was something like 20 or 30 per cent has been removed within that 3 million year period.

DIODATO: So, 70 to 80 per cent might still remain?

MURPHY: That's a number that pops in my head, but like I said, I'm going to have to look it up to know for sure.

DIODATO: Thank you.

MURPHY: Pardon me, let me reiterate. That calculation was a maximum limit on how much. The effort that I made was not to try to calculate the precise oxidation rate, but to set a limit, maximum possible rate, and that includes all the uranium that's been oxidized and departed the system.

DIODATO: Diodato, Staff.

Just help me to understand what that means in terms of how much remains, what's the implication of that?

MURPHY: The implication is that the oxidation rate places a limit on the dissolution of spent fuel. So, spent fuel dissolution is faster than that.

DIODATO: Okay, thanks.

CERLING: Okay, thanks, Bill. And, we'll move on to our next speaker, Ardyth Simmons from BSC, Los Alamos National Lab, Science and Technology Program Work at the Pena Blanca Analogue Site.

SIMMONS: I'd like to thank the Board for inviting me here to this meeting to give a presentation on our plans. From Bill Murphy, you heard a lot about the work that the Center for Nuclear Waste Regulatory Analysis has done, and that Bill himself is continuing.

About 1999, the Yucca Mountain Project decided to do some studies that would look at the possibility for transport in the third dimension by drilling some wells. And, that program is coming to an end right now, with this year, we'll be publishing results of our studies and an update of the Natural Analogue Synthesis Report, and that will be coming out in May.

So, there will be a lot of data in that that I'm not going to be touching on at this meeting. Instead, I'd like to tell you about the plans in the next three years for the work to be continued in the Science and Technology Program that arises out of DOE headquarters.

The team that is involved in this new effort involves three national labs, five universities, and a company. So, it's a larger group of people that have been involved in the past. And, in my presentation today, I'm going to touch just very slightly on the work that's been done to date, go over the objectives of our work in the Science and Technology Project, and a little bit about each of the subprojects.

I believe that the Board has received a copy of the plan that we wrote for this work back in January, and that will provide more details.

As Bill already told you, just to give you a picture of the site and the location, the study area is right about here in Chihuahua, with reference to Yucca Mountain Basin and Range. This is what the Nopal I site mine looks like on this escarpment in Pena Blanca. Here's some statistics about the ages of various events that occurred.

He already talked about that.

And, in our previous work, this is the work that was done up until this year, let's say 2003, the DOE researchers have shown that uranium, protactinium and thorium have remained undisturbed in fractures in the unsaturated zone near the deposit for at least the last 200,000 years, whereas, radium shows more recent open-system behavior.

So, if you were listening closely, you'll detect that there's some differences in interpretation between the results that Bill showed on that one diagram of his, and what our fracture filling studies have shown.

We have collected water samples in conjunction with this work, and we've found that there's been a difference in behavior in radium concentrations, and the relative mobility in the unsaturated zone as compared to the saturated zone. And, we feel that this difference in mobility may be due to differences in either solubility complexation or kinetic effects over long transport distances. So, this is something that we're going to be trying to investigate further.

Now, in 2003, three new wells were drilled, and we've obtained core and cuttings and water samples from those wells, as well as water samples from other neighboring wells. In addition, geophysical logs, description of the core collected from the PB1 well, and characterization of rock samples. This gives you an idea of the location of the wells. PB1, this one right here, is located on what's called the plus 10 level on these various escarpments that I showed you in the previous photo. And, it's right about here where there's this sort of gray aura where you would have seen the ore deposit exposed at the surface.

PB2 is roughly 50 meters away, that same level. PB3 is 10 meters down at the plus 0 level, but also roughly 50 meters distance, and PB4 is an old mine supply well that we refurbished, which is roughly 1 1/2 kilometers away. So, that gives us some additional data.

This is a map view and a photograph of the adit at the plus 0 level. The map shows various locations where we've sampled water, and this collection system has been refurbished. Samples have been taken on approximately a quarterly basis over the last couple of years, but obviously depends on precipitation events as well.

Now, moving to the Science and Technology Project. The objectives for our three year study that we're beginning just now are to evaluate Yucca Mountain total system performance assessment model by testing it against field observations and process model results taken from the Pena Blanca site. A big part of this is going to be the development of a more refined conceptual model than what we have at present. And, we're going to be focusing on both positive, or confirmative types of information, and also things that we might find that may be different or negative.

For example, Bill, in his talk, mentioned that you find sulfite minerals at Nopal I that aren't seen there at Yucca Mountain, and this can have a potential difference in mobility as well.

Some targeted Yucca Mountain questions that we'll be looking at are per cent or volume of active fractures in the unsaturated zone, and the extent of fracture matrix interaction. Transport behavior associated with the adits and drifts. And colloid transport. These are among the questions we'll be asking.

The project has been divided into eight subprojects, and from top to bottom, you can see that the top ones are more characterization oriented, rock and hydrologic properties, seepage, colloids, radionuclide transport, isotopic systematics in minerals. We have this study here, assessment of transport at the prior high-grade stockpile site will allow us to look at transport in a very near-by location. So, it will be a completely different site from the Nopal I mine. But, it should give us some idea of transport in that region, and the materials here were taken from the mine. And, then, moving into flow and transport modeling and TSPA modeling.

Now, each of these topics is explained in more detail in your backup material in the handouts, but I don't have time to go into all of these. I want to show you here, however, how the subprojects are related. These four subprojects at the bottom are the more, shall we say, process oriented, or characterization oriented, and they will provide information to Subproject 4 on radionuclide transport.

Together, Subproject 4 and 6, the one I just mentioned to you about transport at the prior high-grade stockpile site, will provide information into Subproject 7 on flow and transport models. This is a numerical model. And, then, it will roll up into Subproject 8 on TSPA. So, this type of a diagram should look very familiar to you from some of the Yucca Mountain work.

Focusing primarily now on the TSPA aspect of this study, our goal is to use the TSPA model to attempt to predict uranium and technician 99 transport at Nopal I. We are going to sample waters in, we hope, sufficient quantities so that if it is possible to detect technician 99, we will be able to. At the present time, we don't have any data on it.

But we will use all the ground truth that we've collected from the more characterization oriented studies, calibrate the model to Nopal I, evaluate its sensitivity to uranium solubility, infiltration rate, dissolution area, and distribution coefficient. And, I'm using this in the same sense that Bill did previously. And, then, scale the results to Yucca Mountain and compare it to improve confidence in

TSPA predictions.

This is a working conceptual model at present, and it's very preliminary and very simplified. Here, you see the ore body, and it's not particularly to scale. The estimated water table, now it's not estimated anymore actually, beneath the PB1 well, the depth is about 238 meters to the water table. We'll be looking at precipitation and infiltration in a more quantitative sense than we have previously, and trying to get an estimate of transport from the unsaturated zone to the saturated zone, as well as getting a regional picture of groundwater flow in the saturated zone.

So, here are some of the steps that are part of that process with TSPA. I guess I've already mentioned some of them in the context of that previous diagram. But, including precipitation, inventory, flow through the ore body, release from the ore deposit, groundwater gradient. We're going to be getting some water level data periodically from the four wells I showed you, plus seven others in the region. Groundwater flow of contaminants. Here, I mean the uranium series nuclides. Setting up a Nopal I simulation using the same code as is used by Yucca Mountain TSPA, that's GoldSim, predicting the transport of Tc-99, as well as the other uranium series products, not the other, the uranium series products, and repeating the analyses for these other daughter radionuclides of uranium.

So, within our first year, and we have about six months left in that right now, these are the tasks that we're going to try to accomplish. Many of these continue into the second and third years, and we have building on activities in those second and third years. But, most of the characterization work for subprojects 1 through 4 will begin this year, and in the case of the rock properties and the seepage and the colloids work, much of that will be completed.

Now, this slide shows what we anticipate to be able to deliver not this year, but at the end of the three year project. In our reports, and we'll have some peer review publications, certainly, we'll be producing a rock and fracture properties data set, an archive of water and rock analyses, standards for mapping U-series elements in minerals, a three dimensional gamma spectroscopy map of this prior high-grade stockpile site, a hydrologic gradient and potentiometric map, and the TSPA analysis.

The rest of this material is backup, and if you have any questions about it, I'd be glad to try to answer them perhaps later as to the specific activities of the project. I've sort of glossed over a lot of the details right now.

CERLING: Okay, thank you. Some questions from members of the Board? Rein?

VAN GENUCHTEN: A mixture of this is, I guess, future activities; right? I'm curious what kind of models you envision for the unsaturated zone. Are you using any existing models, maybe some of the ones that are being used at Yucca Mountain? What's your plan?

SIMMONS: Yes, for the unsaturated zone model, this will--let me see if I can go back to the little--here, this Subproject 7 will be a numerical flow and transport model, and it will include both the unsaturated and the saturated zone, and we will be using TOUGH-2 model for that, for both the unsaturated and the saturated zone. So, the same sort of tools will be used as we're using for Yucca Mountain now, and the same sort of methodologies, recognizing that we will not have the same level of detail for characterization of all the parameters at Pena Blanca as we do for Yucca Mountain, because we're not trying to do a parallel site characterization study. But, we will be using the same approaches.

VAN GENUCHTEN: All right. Are you doing initial kind of modeling studies? I mean, you're already electing data, you know how those data fit in with the models?

SIMMONS: Yes. We've been able to benefit, obviously, from the fact that the Yucca Mountain Project has already, for several years, allowed us to collect data on this site. And, as we've gone along, we've been comparing our state of understanding at Pena Blanca to Yucca Mountain. We will be making some predictive models at the beginning of this activity also, and calibrating and updating them as we go along.

CERLING: Dan Bullen?

BULLEN: Bullen, Board.

Actually, you just led into my question. You said you were going to do some predictive models. And, along those lines, what do you think are the most significant differences between the two sites, and how will you deal with them as you try to develop your models and analyze your data?

SIMMONS: Well, certainly, you have a scaling issue to start out with. So, you have to deal with that. Also, at Pena Blanca, we're dealing completely with the natural system. So, there's no waste package or anything like that there, and that has to be recognized. Now, that said, you know, as far as the differences between the two sites per se, we have a number of different minerals that are present at the Pena Blanca site, which we wouldn't expect to have in spent fuel, and I think Bill already touched on that.

And, another thing that I wouldn't characterize necessarily as a difference, but it's a dearth of understanding at Pena Blanca, and that is how the neighboring uranium mines, this is in a uranium mining district, how they may have an effect on the groundwater system. So, I think it's going to be challenging to uniquely identify the signature that could be derived from Nopal I, and, in an analogous sense, Yucca Mountain is not in that type of an environment.

BULLEN: Thank you.

CERLING: Richard Parizek?

PARIZEK: Parizek, Board.

Colloid experiments that you plan, can you elaborate a little bit on those, because it really is kind of a necessary subject matter area, because you're in that unsaturated zone, but you could also do colloidal work in the saturated zone. Perhaps expand on your experimental design.

SIMMONS: Sure. The colloid study is going to be done in kind of, let's say, it will evolve as we go along. In the first year, we will be sampling the waters for the determination of the colloids that are present in the samples that we take. We'll do that for samples that we derive from the adit in the unsaturated zone, as well as the water samples that we take from the wells.

What we may do in the second year, and we will be planning this as we go along, we may do some testing using microspheres to try to see about transport pathways for colloids, and we will be doing, if we detect, which we probably will, natural colloids in the waters that we collect, we'll be doing some further characterization of the colloidal particles as to their compositions. Are they natural colloids? Are they colloids that, thorium colloids, for example, or, you know, what their constituents are?

So, then, based on that information, we'll be able to put that into a radionuclide transport model that will include colloids. But, that step depends on what we find in the previous tests.

PARIZEK: Parizek, Board.

Again, with regard to the stockpile, that's on alluvium? That was stockpiled out in the desert environment alluvium at a known date. So, you have leaching I guess of this ore storage pile?

SIMMONS: Exactly. It's not in alluvium. It was actually stockpiled on the bedrock on that surface.

PARIZEK: Okay, so different. It was another place down the road where there was stuff stockpiled.

SIMMONS: Right. It wasn't that site, though. But, you're absolutely right. We have a very firm date when this stockpile took place. So, we have a starting point, and we can see how much has been leached over that period of time since the mid Eighties.

CERLING: Okay, thanks, Ardyth. I'm going to try to keep on schedule, and we have a substitute talk right now. So, Russ Dyer is going to give a short presentation at this point, and then we'll move on and get back to our regular schedule.

DYER: Thank you, Mr. Chairman.

I appreciate the indulgence of the Panel for allowing us to insert this presentation. My task is the respond specifically to one of the questions that were posed for this meeting, and to set the stage for this afternoon's remaining presenters.

The session organizers requested information about the median travel time for a molecule of water in the saturated zone and unsaturated zone from the repository horizon to the regulatory boundary. That's not something we routine calculate. And, the reason is that such a calculation is not a meaningful parameter for our risk assessment calculation, nor is it part of the regulatory basis.

Several of the subsequent presenters will address radionuclide transport models, and abstractions that support the existing Total System Performance Assessment for License Application.

I want to make a point that these presentations do not directly address the expected travel time of water molecules, either in the unsaturated zone or the saturated zone.

Now, in order to be responsive, we were trying to figure out how to do this, a non-sorbing, diffusing radionuclide with a load effusion coefficient, like technician, could be used to approximate the expected travel time of a water molecule. And, in the past, we've done a couple of examinations looking in both the UZ and the SZ at such an approximation. We haven't redone these calculations in a while, but examination of current information suggests that the results using this approach would not be significantly different from those developed several years ago.

And, this is what we get. And, if I could get the pointer here. There are three breakthrough curves on here, and let me talk a little bit about this curve, or this suite of curves.

First, this is looking at travel time from the repository horizon to the 18 kilometer compliance boundary. This is a deterministic calculation. Of course, all the models that go into the TSPA have a range of parameters. For this, what we did was pick the single value best estimate for each of the independent input parameters.

A couple of other caveats. This uses the current present climate, and it allows for matrix diffusion. Of the pertinent points, the black curve is the saturated zone curve. The blue dashed curve is the unsaturated zone curve, and then the total is this red curve here. And, if you look at, say, the median value, that would be of about 50 per cent here, it's about 10,000 years. There's the time scale on the bottom. 10,000 years for a cumulative travel time, about 8,000 to 9,000 years for the unsaturated zone, and a little over a thousand years, 1,200 or so, for the saturated zone.

Now, just to set the stage for the following presenters, Jim Houseman, George Moridis, and Bruce Robinson, their presentations will use radionuclide breakthrough curves to illustrate predicted transport behavior of the calibrated UZ models and abstractions. These radionuclide breakthrough curves do not represent expected travel time of water molecule. The breakthrough curves do portray a range of parameters to characterize uncertainties, and these breakthrough curves are developed with conservative inputs to fully assess the impacts of uncertainty.

And, my task is complete. I've set the stage for the following presenters. Questions?

VAN GENUCHTEN: In your Slide 4, it's a deterministic prediction, which model did you use for that?

DYER: I'm going to have to look for Bob Andrews to stand up and help me here.

ANDREWS: Yes, these calculations, this is Bob Andrews, BSC, these calculations were done some three years ago, I want to say, using the calibrated site scale unsaturated zone flow and transport model that you're going to hear a little bit later that's been updated a little bit from Jim and others, and the same for the saturated zone.

As Russ said, it's a deterministic case. So, it was the expected value realization from a suite of a range of realizations that the subsequent presenters are going to talk about. So, it's one case.

BULLEN: Bullen, Board.

Along the lines of the same type of question, you said it was a single value best estimate, and you mentioned that it had matrix diffusion associated with it. But, in a transport case, I mean, if I was looking at a plume of these water molecules, did you have dispersion also, or this is just a slug flow kind of characteristic?

DYER: I don't think it was a slug flow.

BULLEN: Sort of a slug flow, kind of pipeline flow?

ANDREWS: I mean, it was a spatially distributed, Bob Andrews again, spatially distributed source region at the UZ across the whole repository domain, similar to what you're going to see later on. And, so, there are different flow paths, if you will, associated with that spatially distributed source region. And, the same is occurring in the saturated zone for the particles released in the saturated zone. So, from that sense, there's a spatial distribution of flow paths, which ends up having the dispersive type phenomena, as you're describing.

BULLEN: Okay, thank you.

NELSON: The way these are treated by, are they just added together, those two curves?

ANDREWS: Yes, I think they were sampled separately and then added.

NELSON: Now, is there not an interdependence between the two?

ANDREWS: I believe in the way this one was done, although I'd have to verify it, to be honest with you, is they were sampled independently.

NELSON: Is there not an interdependence? I mean, in fact, you have flow paths coming down through the unsaturated zone, spatially distributed, contacting a spatially variable saturated zone, they would depend, one upon the other, would they not?

ANDREWS: They could, yes. In the saturated zone, I believe, and Bill Arnold or Stephanie can correct me if I'm wrong tomorrow, they had four regions that they were capturing, if you will, the particles, and then releasing them from the saturated zone the rest of the way through to the 18 kilometer compliance boundary. I'm not sure that there was any correlation, if you will, which is I think what your question is, between where in the saturated zone the individual particle trajectories arrived, versus how they were added to the additional transport time in the saturated zone. I would need to evaluate how the calculation was actually performed.

NELSON: Fair enough. I'm sorry, that was Nelson.

CERLING: Okay, thanks, Russ, for getting this kicked off. And, the last presentation before the break will be by James Houseworth, Conceptual Models and Independent Lines of Evidence for Evaluating DOE Unsaturated Zone Model Calculations.

HOUSEWORTH: Thank you.

I'd like to acknowledge that this presentation was put together jointly between me and Bo Bodvarsson, and also acknowledge the work of numerous scientists on the Yucca Mountain Project, which this talk is based.

The outline of the talk, the subject matter here, we'll be going through a series of conceptual models, and along the way, I'll be discussing the independent lines of evidence for those conceptual models. Starting off with future climate projections, which have a major impact on the hydrology in the unsaturated system. Then, we'll talk about models for percolation and runoff for net infiltration. Then, the geology for the unsaturated zone in terms of how that's represented in the UZ models. Then, I'll get into some issues related to flow and transport in fractured rock, both in terms of fracture/matrix interaction and representation of flow in fractured systems.

And, then, later, I'll be going over some topics that relate to some of the larger scale effects in the UZ flow model, episodic transient flow and associated fast flow paths, as well as larger scale lateral flow. Then, I'll be going into some topics that are more directly related to transport phenomenon, particularly the matrix dominated flow patterns in the Calico Hills non-welded vitric that lies below the repository horizon, the topic of matrix diffusion, which has a major effect on transport. Also, some issues related to the radionuclide source term, how radionuclides initiate transport in the rock after coming out of the emplacement drive, and tie that in with the drift shadow concept. Then, I'll put this together, in terms of the main sensitivities found for transport time of a passive tracer, and summarize with conclusions.

So, the main processes involved in the unsaturated flow system are, first of all, climate, which sets the precipitation and temperature, which is a very important control then on infiltration. Infiltration is primarily balanced between precipitation and evapotranspiration, with smaller elements of the water balance being runoff and net infiltration.

The flow then enters the unsaturated zone, and goes through a series of rock units, fractured rock units, and the character of that flow changes rather significantly as we move between the different units.

There's also a lateral flow phenomenon that is anticipated, based on the modeling work and the field data, both above the repository and more significantly, below the repository.

Perched water bodies are known to exist below the repository and are a major factor in the overall lateral flow process below the repository horizon. And, the effects of lateral flow also lead to an enhancement of flow in faults, especially below the repository horizon, flow and transport in parts of the repository are dominated by faults.

A key concept in the climate model is the climate cycles, and Saxon Sharpe went into this in great detail this morning, so I won't go over this in too much detail. The graph in the upper right shows the cycles of climate as found in the delta oxygen 18 record for Devil's Hole. And, the correlation of that cycle, those 100,000 year cycles, with the earth orbital cycles is a key piece of information that supports this idea of a 400,000 year climate cycle.

I'd point out that additional information is needed for describing the specifics of the climate magnitudes. In terms of the fossil record that was taken from the ostracod data at Owens Lake, there's, first of all, if you look at the bottom of the graph, you'll see that during the modern climate, which starts about 400,000 years ago, there's very little growth of any ostracods in the system.

And, then, as we move beyond that time, we come into the monsoon climate, and in that climate, there's several species which show strong growth patterns. And, then, after about 2,000 years, we end up here in the glacial transition cycle of the climate, and that is dominated by the--so over here, you can see that this is a strong ostracod signal of the glacial transition climate.

Then, temperature and precipitation ranges associated with this Owens Lake data are used to select analog climate sites and to represent future climate. And, this map shows the sites that have been used for these analog climate data. And, Saxon went into this also in a fair amount of detail, so I won't go over that here.

The most important thing to recognize is that these upper and lower bound analogs define the climate uncertainty, and that is propagated into the UZ flow and UZ transport models. And, it's an important source of overall uncertainty in the UZ system.

So, the percolation and runoff for net infiltration are two of the elements of the infiltration model that are treated using approximation to the physical processes that are typically used.

Percolation is treated as a vertical, piston flow process in this model, which, to a large extent, ignores the unsaturated flow and capillarity of the system, with the exception of a residual that's defined by the fuel capacity.

Runoff patterns are shown here in this diagram. Wherever runoff is generated, then it flows from cell to cell based on the nearest neighbor, the lowest elevation nearest neighbor, and that is a geometric approximation to the runoff process.

The durations of this runoff process are based on runoff observations at Yucca Mountain, which are very short-term, and in the model are set at two hours for the summer storms, and 12 hours for winter storms.

The average present-day net infiltration ranges from approximately 1 to 11 millimeters a year, with an expected value of about 4 millimeters a year. And, the evidence for this, as a reasonable prediction for infiltration, comes from geochemical data and global temperature data.

So, here we have the chloride data, which is shown from the ESF, and the model was run as a chloride mass balance type of calculation, and shows a reasonable agreement at least for the present day mean, which is the red curve, and the present day upper infiltration scenarios. The green curve, which is the low infiltration scenarios, those follow, but off that chloride data.

The global temperature data, which is shown in the lower curve here, was taken from a borehole H5, shows also reasonable agreement of the borehole temperature profiles are sensitive to the percolation flux, and basically provide confidence in the infiltration model.

The geology controls the character and flow patterns in the unsaturated zone, and, so, it's important to capture that in a realistic way. The geology has been defined through extensive surface mapping and trench studies. And, the stratigraphy of tuff layers have been evaluated from over 60 deep boreholes, and more than 10 kilometers of tunnels. These two diagrams give an idea of the level of detail that's captured in the 3-D UZ flow and transport models.

So, this information, in combination with detailed hydrologic measurements, have resulted in hydrologic stratigraphy with 32 hydrogeologic unit. Properties within the units are homogeneous, except for zeolitic alteration. So, you can see, for example, in this unit, through the Topopah, we have homogeneous properties through those layers.

The major faults are also included as vertical or inclined discrete features, and you can see the green lines that run along this plane view of the UZ grid that includes these features.

Vertical dimensions in the repository, or throughout the model, actually range from 1 to 20 meters, of a 5 meter grid dimension within the repository horizon itself.

The horizontal grid dimensions in the repository are on the order of 100 meters, and outside of that, the horizontal dimensions are somewhat larger.

Grid sensitivity studies, which will vary these dimensions by up to a factor of four, found the variations in transport breakthrough times have been on the order of 10 to 20 per cent. That provides some confidence that the level of detail in the griding is sufficient.

Another issue that's related to this assumption in the model of homogeneity within the layers has been investigated using a fine scale two dimensional cross-sectional model. And, these color contours over here show the geostatistical model that was used to populate this fine grid model with heterogeneous properties for matrix permeability, matrix alpha, the capillary pressure parameter, and for the fracture permeability. These geostatistical variables were taken from information derived from different calibration runs.

The results of the model are shown down here in this flow right in here. There's the matrix flow. And, Case A is a case where we use the same assumption of homogeneity within the units. Case B is a case where only the fracture permeability is heterogeneous. And, Case C allows full sets of parameters to be heterogeneous. And, what's found is that in the matrix flow case, when you have a change in just the-- or heterogeneity in just the fracture permeability, the flow in the matrix is affected very little. When you have all three varying, then you do get some variations occurring within the matrix flow patterns.

In the fractures, however, there's really very little variation for any of those cases showing insensitivity to this kind of heterogeneity. This is also studied in terms of the effects on transport, and this graph shows the breakthrough curve for these three cases, and an additional case. Then, Case A, B and C, as I described, Case A is the base case, and here's Case C, the dotted curve, where we have all three parameters varying. And, when you see that there is some sensitivity in the early breakthrough, the sensitivity is not large. For example, in comparison with this curve where we varied the matrix diffusion coefficient in Case E.

And, another--this graph on the right also provides kind of a calibration in terms of the range of uncertainty in the model to be compared with this type of uncertainty. This shows the breakthrough curves for technetium under a low, mean, and upper climate scenarios for present day climate. So, that's basically the climate uncertainty.

Given that we're talking about the fractured rock system, with a porous rock matrix, there needs to be a conceptual model that connects the flow and transport behavior in the fractures with that in the matrix. And, this series of diagrams shows the connection, they're connection diagrams for fracture and matrix, and different conceptual models. And, Alan Flint went over some of these earlier when he was discussing some of the historical developments in terms of a conceptual model.

We did begin with an equivalent continuum model, which assumed equilibrium between the fractures and matrix, and, so, there's only a single variable required to describe the flow conditions in the fractures and matrix, because of the equilibrium assumption. And, the black arrows here denote a global flow pattern then through this fracture matrix equivalent continuum system.

However, capillary disequilibrium is expected based on the fact that we do believe that there's fracture flow occurring, in conjunction with an unsaturated matrix. And, furthermore, the perched water and pore waters in the matrix appear to be in chemical disequilibrium, again, leading to the idea that the equivalent continuum model may not be sufficient.

Another conceptual model is this dual-porosity model, which allows for fracture/matrix disequilibrium. However, as shown here, here's the red arrows are the fracture matrix interaction, black arrows are the global flow. The dual processing model does not allow global flow in the matrix, and this was never considered a particularly good model for Yucca Mountain where global flow is expected in the matrix, and in fact, it's dominant in some units.

An extension of this then is the dual-permeability model, which allows non-equilibrium fractured matrix exchange and global flow in both the fractures and the matrix. And, this is the current conceptual model used.

One issue that remains with this is that it may under-estimate fracture/matrix interaction for transient problems. And, to address that particular type of issue, there was a more complex model called Multiple Interaction Continuum Model, or MINC model. And, this model allows for disequilibrium and also a more discretized representation of the fracture/matrix interaction, allowing for a better representation of these kind of conditions, particularly for transient problems.

Finally, discrete fracture model is probably the closest to the physics of the system, but would require data and computer models that are simply not available at this time for a mount scale model.

And, I just wanted to point out what the effect of the MINC versus the DKM models have on transport, because they are fairly large. If you focus, this graph has a number of curves with different sensitivity calculations, if you focus on the red curve, which is the curve for the breakthrough DKM model, and the black curve, which is the breakthrough curve for the MINC model, you see that there is, in fact, a fairly large difference in breakthrough behavior. This is a two dimensional cross-sectional model, which is consistent with what we would expect from these different conceptual models.

The actual differences may be exaggerated, however, because although the DKM model has been calibrated to the flow date, the MINC model was not. And, furthermore, in the 2-D model, we found, as compared to higher dimensional 3-D models, differences tend to be exaggerated, based on these kind of different process descriptions.

The dual permeability model requires a treatment of unsaturated flow in fractures, and that is a continuum representation. This is still something of a research topic, primarily because there isn't a great deal of data on it. It's actually the flow and fracture networks.

Small scale discrete fracture network models, however, have been used to give us a theoretical look, essentially, at how fracture network behavior may compare in the discrete system with a continuum representation. So, here, we show a discrete fracture model, two dimensional discrete fracture model, that was used to investigate the capillary pressure of relative permeability characteristics of the system. And, this was investigated by placing constant capillary pressure conditions on the upper and lower boundaries, and those load conditions on the side boundaries.

And, then, by changing the capillary pressure, you can evaluate the capillary pressure curve for this kind of a network. And, this was fitted to a van Genuchten expression for the capillary pressure and found that it did a fairly good job in matching the data.

Then, the parameters from that were then taken over to the relative permeability curve, which then had no further adjustable parameters, and this lower gray line is the relative permeability curve that results, which under estimates over most of the saturation range the relative permeability. However, it does a fairly good job at low saturations, and this is the range of saturations where the model in the natural system is expected to primarily reside.

There are some field data, and this is some of the same data I believe that Alan Flint showed for the disk infiltrometer experiments conducted in bench tests in the south. And, what this shows is that when you put this system in place and establish a steady state condition under controlled capillary conditions, the relative permeability curve drops off as a function of capillary pressure.

One thing you don't get from this kind of an experiment is how these things vary as a function of saturation. And, another caveat on this is that the test data is limited to what we believe are higher saturations, or at least under capillary pressure conditions that we suspect are at higher saturations.

Well, another element that has to be captured in the fracture network model, or in the fracture flow modeling, is that preferential flow in single fractures, and in fracture networks, have been observed in the laboratory and field tests, so there has to be some way to account for this type of phenomenon in the flow model. We don't expect the flow to just proceed uniformly through the fracture networks.

To account for this, there was a modification of the van Genuchten formulation, which is called the active fracture model, and the active fracture hypothesis, which is shown down here, is that the active fractures is proportional to the fracture saturation to an empirical power, gamma. And, as that model is implemented in the relative capillary pressure curves, what we see is that the, as the gamma value runs from zero to .9, of course, the value of zero gives active flowing fracture of one. So, that's just uniform flow that would represent the original van Genuchten curve. And, as the flow is essentially packed into fewer and fewer of the available fractures, this capillary pressure drops, or heads towards a condition where it would be more like a saturated condition, which is what we would expect.

In terms of the relative permeability curves, there's kind of an interplay between a reduction in the number of fractures that are flowing, and yet the fractures that are flowing have a higher saturation, the net effect of those two results in an increase in the relative permeability with this flow focusing. So, you get higher effective permeabilities with the flow focusing.

Probably the most significant effect of this overall active fracture model is that it does affect the fracture/matrix interaction. What we show here, it's a plot of the fracture/matrix interaction factor, which is a function of the wetted fracture/matrix interface area, and the flowing fracture spacing. And, what this shows is that as we move from a gamma of zero, shown up here, down to a gamma of .9, which is a very high gamma, there is a significant reduction in the fracture/matrix interaction. And, likewise, there's a reduction in the fracture/matrix interaction factor with saturation.

There have been some sensitivity studies carried out to look at the effects of this parameter, gamma, on radionuclide transport. These studies were conducted with the 3-D site scale flow model and transport model. And, the red curve is the calibrated model curve for breakthrough. The green and the blue curves show the effect of changing gamma, a reduction by a factor of 1/2. So, as you reduce gamma, it reduces the--the one curve reduces gamma in the Topopah only, and then the other one reduces it in all the units below the repository horizon. What you find is that most of the effect is seen by changing the gamma in Topopah.

And, this is a result of the larger scale flow and transport patterns, which focus most of the transport into fault zones below the Topopah, or it's moving through the Calico Hills non-welded vitric, which is matrix dominated flow and transport system.

The active fracture model is needed to match water saturation and potential data. What we're showing here is a match between the flow model and saturation data at SD-12. And, without the reduction in the contact essentially between the fracture and the matrix, it's very difficult to match the Topopah zones in particular.

Independent evidence for this active fracture concept comes from frequency of secondary calcite coatings on fractures in the Topopah Spring welded unit. In those units, the fracture coating frequency is on the order of about 10 per cent, and the active fracture model for current climate, or even for future climate conditions, gives values of flowing fractures, the fracture of flowing fractures, in a similar range, roughly in the order of 10 per cent.

Now, I'll be talking about some of the larger scale flow patterns, mountain scale flow patterns, that relate to episodic flow and large scale lateral flow.

The infiltration, which is a very transient process, is expected to penetrate through to the canyon welded unit as a fairly episodic transient type of phenomenon. But, upon entering the Paintbrush non-welded unit, the flow is homogenized, both temporally and spatially. And, this is due to a high permeability matrix of the Paintbrush unit's walls, its capillary characteristics.

Some lateral flow is expected in this model. We'll go over why we believe this is true in the UZ flow and transport models.

In the Topopah, then there's a relatively uniform steady flow pattern that passes through the repository horizon, then encounters in the northern part of the repository, perched water zones, which represent permeability barriers. And, at those locations, there's clearly a factor that would drive lateral flow and flow focusing in the faults.

In the southern part of the repository where the Calico Hills is not altered, the process is dominated by this Calico Hills non-welded vitric matrix flow pattern.

So, episodic transient flow is the initial pattern that we expect in the upper part of the mountain. However, model calculations demonstrate that these transients are damped out by the high permeability and capillary properties of the PTn.

These set of graphs show a cross-sectional model taken through here, which is just a small piece of this overall cross-section, was used for this transient flow study. And, down here, what we see are the influx at the surface, which are these black spikes, which are an infiltration of 250 millimeters per year, of 5 millimeters per year, all entered into the unsaturated zone in a period of one week. So, you have these 50 year pulses that are going into the system, and the flow response below the PTn is shown here. Both a 1-D and a 2-D model were run here, and both show fairly little disturbance based on this rather highly transient boundary condition.

And, along here, this shows the flux pattern coming out of the PTn as a function of the cross-sectional distance, and it shows again similar, with time, you get some perturbation to the flow, but it's not particularly significant.

So, the evidence that we have for this damping out of transient flows comes from some of the isotopic data that have been taken, both above and in the repository horizon, and some information from below as well.

Carbon 14 data, which is shown in this graph, shows the age of the pore waters are on the order of a few thousand years. And, chlorine 36 data, which have some controversy associated with them, but still suggest that the fast flow paths, at least are associated with faults, shown here, or low angle features in the Topopah Spring welded unit. So, it looks like there's not a pervasive pattern of episodic transient flow penetrating the PTn.

And, furthermore, lack of bomb pulse in chlorine 36 and perched water suggests that the quantity of fast flow is small.

Another significant, the flow pattern that evolves out of the UZ flow model is a large scale lateral flow. In the PTn unit, it has been found that capillary barrier between different sublayers of the PTn do generate some degree of lateral flow. This shouldn't be looked at as a complete barrier to that flow, but it's really rather a leaky type of barrier where there's lateral diversion, and, yet, quite a bit of the flow still penetrates through the PTn into the underlying repository horizon.

So, in some sense, this is consistent with what Alan was presenting earlier, although the actual scale of lateral flow in terms of the distances are somewhat larger in this model as compared to what Alan was presenting.

And, this is a two dimensional model in which we show the patterns of infiltration, and then the patterns of flow coming out of the bottom of the PTn. And, what you see is that there is a relatively large degree of smoothing of the flow created by this lateral diversion. This shows the two layers where significant lateral flow is occurring, and that this flow moves over two fault zones, and then in the two dimensional model, would stop at that point, and is forced downward.

To a large extent, the water does not enter the fault zone directly, though, because of the capillary barrier presented by the fault itself.

This plot shows the sensitivity of the lateral flow to infiltration, and the parameter used to demonstrate lateral flow here is the flux in fault zones, or near fault zones, which is shown on the pink curve. As the infiltration increases, the capillary barriers break down, and the level of lateral flow decreased.

Chloride data is one of the primary sources of information that we are using as evidence for lateral flow. This profile was taken at SD-9, and the dots represent the measured chloride values. What we see is this decrease in chloride concentration as we move down through the PTn. And, the green, red and black curves are the current baseline model in which we have lateral flow occurring in the PTn. And, the base case, or the mean case, shows that it fits this in an approximate way.

The dashed curves are an alternative model which we do not have much lateral flow in the unit, and you, therefore, don't see much of a decrease or an effect of lateral flow in that profile.

And, similarly, there's data taken from the ECRB, and this data shows, the dots, is compared with both the baseline model, which contains lateral flow, the solid curves, and the dashed curves, which do not include a lateral flow component in the PTn. And, there's a slightly better fit of the data with the model containing lateral flow.

There's evidence for perched water from several boreholes at the site. And, the existence of this perched water, from this, we can infer that there's a permeability barrier at those locations.

Lateral flow, due to these permeability barriers is expected below the repository horizon, and these primarily lie along the low permeability zeolitic units in the northern region of the repository.

The main effect of this is that this diversion tends to minimize contact of flow or transport coming out of the repository with the zeolitic tuffs.

These three contour plots show kind of the progression of the flux field as you move from the surface. Here's the infiltration map. Then, here's the map of flux at the repository horizon. What you see is that there's some higher infiltration zones kind of along the western edge, and that kind of gets smoothed out, and so you have a more uniform pattern of percolation flux at the repository horizon.

Then, below the repository horizon, there is almost an exclusion of flow in the north, where most of the flow has been focused in the faults. And, in the southern region, where there's the Calico Hills, is primarily unaltered vitric rock. You have primarily downward flow, matrix dominated process.

These two curves present kind of the impacts of this lateral flow on radionuclide transport. There's some other things going on in these curves, but if you focus in this plot on the right, you have the blue and red curves, which are the two models for flow with lateral diversion in the PTn, and without lateral diversion in the PTn. And, what you see is that the effects on transport are relatively minor.

In this plot, there were some different perched water models that were investigated, and for the present day climate, it's this trio of black, red and blue curves, solid lines, for a non-sorbing tracer.

The one curve that does show some significant differences is what's called the no-perched water model, in which we simply ignore all the perched water and let everything go vertically, and that did show some more rapid breakthrough. But, the two models that were consistent with the field data showed very little difference in terms of transport behavior.

Now, I'll be talking about processes that are more important for the actual transport processes below the repository. These are the flow behavior in the Calico Hills non-welded vitric, effects of matrix diffusion, the source term, drift shadow effects. And, then, sorption and colloids I won't go into, but will be covered by George Moridis in the next talk.

Busted Butted field test sites, about 8 kilometers southeast of Yucca Mountain, presents an outcrop of the Calico Hills vitric unit, which was tested over the last few years. The tests were conducted using multi-tracer solutions of water and tracer injection, and water and tracer collection, as well as geophysical measurements, including ground penetrating radar, and electrical resistivity tomography.

And, one of the main findings of these tests was the definite matrix dominated flow patterns that were found. This upper picture shows fluorescent dye that was injected into a single borehole, and injection points are in the middle. So, what you can see is that the injection was dominated by capillary phenomenon, and spread out more or less uniformly from the borehole without substantial effects of fractures, or of gravity.

Then the Phase 2 tests show injection into a series of boreholes that activate a larger portion of the block, and these injection holes are on this part, and this is a GPI image of that test. The red shows the flow that was injected, essentially. And this series shows the time development of that flow pattern.

What you see is a strong matrix type flow pattern, where the water is pulled laterally, and even up, and this, again, shows a strong porous media flow behavior.

Investigations were conducted at Alcove 1 in terms of flow and transport behavior in welded tuffs. Alcove 1 is the first alcove in the ESF which lies just 30 meters below the ground surface, as shown in this figure. Then, the tests were conducted by ponding water over the alcove and then collecting the water in this alcove.

The tests were initiated with water and were allowed in two phases, and the flow patterns were allowed to stabilize, and then tracer was added to the injected water, Lithium bromide tracer.

One of the observations from the surface part of the test was that the water uptake rates were on the order of 30 millimeters per day, indicating, as what Alan Flint discussed earlier, that the surface fractures are significantly less permeable, because this rate would be much higher if it was just in the open fractures.

The data was then used to calibrate a flow model. The MINC model was used in this case, because as I was discussing earlier, it's believed to be a better model for transient phenomena, and, so, we used this to match the transient flow and transport experiments in this alcove test.

The calibration is shown here, so we have the data in red, and the calibrated flow model shown in green, which can match most of the behavior of the water collected. This is the seepage data that entered the niche.

Then, there was the transport test, and what we show is the transport breakthroughs, these green dots, and there were three curves here that checked the sensitivity out with the transport predictions relative to, in this case, tortuosity factor, which is something that affects matrix diffusion in general.

And, what was found with it was there was a significant amount of matrix diffusion that was needed to fit, in fact, the additional fits with even higher fracture/matrix interaction was found to fit this profile better than the existing plots here.

The modeling studies have been conducted with regard to how flow and transport occurs in the vicinity of a waste emplacement drift. For drifts without seepage, we get this kind of a flow pattern, where the flow is diverted around the drift, leaving the zone beneath the drift relatively dry, and analyses of the transport behavior in this kind of system have shown that the radionuclide transport is considerably slower on exiting the drift in the drift shadow environment.

There's two main effects that are significant for the drift shadow problem. One is that radionuclides leaving the drift predominantly enter the rock matrix. That's because the shadow is much stronger in the fracture continuum than in the matrix continuum, so you still have a lot of matrix water below the drift, but very little fracture water.

Secondly, the radionuclides enter a zone in which fracture flow is negligible. It's not exactly the same as this. This just says where things start, but this says the kind of hydrologic environment that the radionuclides enter. So, it turns out the first item may be the most significant.

This part which I showed earlier now shows some of the effects of this matrix release. So, the red and the blue curves are the base case and alternative models for release into fractures. The black and the green curves represent the same calculation, but releases into the rock matrix. So, there's a significant sensitivity to the initiation of transport, however, there was no drift shadow per se in these curves. This was done just releasing into matrix in an unperturbed flow system.

I should point out that this type of effect will be included in the TSPA, but the full drift shadow effect has not been worked out such that it could be included in the TSPA. But matrix release is something that will be included in TSPA.

So, kind of in summary, the main sensitivities that we found in transport were, first of all, climate, as shown here, has a major control on uncertainty for tracer transport. This shows the variation, tracer transport times for technetium under the different lower, median and upper bound climate scenarios.

Fracture/matrix interaction also has a major effect on the differences in transport, and at the present time, is modeled both in terms of the active fracture parameter, but also in terms of diffusion coefficient. Uncertainty in the diffusion coefficient is included in the TSPA model, however, uncertainty in the active fracture models is represented through bounding values at this point.

And, then, the effects of the radionuclide, how radionuclides initiate their transport, is shown here, which is the last slide I just went over. It shows again this relatively large effect.

So, in conclusion, we have effects of the key conceptual model for climate is supported through the paleoclimate data and correlations with the earth orbital behavior.

Predicted net infiltration rates using the water balance model and some of this process simplifications used in that model have been found to be in general agreement with percolation data, including chloride data and borehole temperature data.

Representation of heterogeneity based on hydrogeologic units is generally found to be appropriate for flow and transport at the mountain scale. That was based on those sensitivity studies that I showed, both in terms of good sizes and smaller scale heterogeneity.

The dual-permeability method is the baseline modeling. We have captured the main features of flow in fractured rock. But, it likely does under estimate fracture/matrix interaction for radionuclide transport.

The unsaturated zone flow in fractures using the van Genuchten continuum relationship appears to be adequate for low fracture saturations. This is based on the theoretical study using the discrete fracture approach. However, the data at low water saturations, it's currently not available. In fact, there's very little data on flow in fracture networks.

Active fracture model accounts for reduced fracture/matrix interaction, and is found to be qualitatively consistent with the fracture coating data.

Episodic transient flow and fast flow paths are likely playing a minor role in the overall flow at Yucca Mountain, and the line of evidence suggesting that this is true, is from the carbon 14 and the chlorine 36 data.

Large-scale lateral flow in the PTn is consistent with chloride data. However, it's not, again, not a complete diversion of flow, and, in fact, is found to have relatively limited impact on radionuclide transport.

The matrix-dominated flow in the Calico Hills non-welded vitric is shown to be consistent with the hydrologic properties and observations at Busted Butte.

Matrix diffusion played a significant role in transport through welded tuffs, as shown in Alcove 1 tests, and we have additional tests at Alcove 8 and Niche 3, which show the same basic conclusions. At least under these kind of stress conditions where we're putting water in at high rates, we seem to get more matrix diffusion than we really anticipated.

Transport times are sensitive, found to be sensitive to infiltration, climate uncertainty, essentially, fracture/matrix interaction, the diffusion coefficient in the active fracture parameters, and the initial conditions in terms of initiation of transport in the fractures or in the matrix.

So, that's the end.

CERLING: Questions from the Board? Rien?

VAN GENUCHTEN: Yeah, I have a few questions. The active fracture model, actually, maybe you can go back to Page 31, or Slide 31. I think I completely agree with the basic philosophy, we see that in soils also, that in micro-pores, you have a lot of preferential flow within micro-pores. And, the same I would find occurs in fractures.

The next question I guess would be how do you implement that, and, so, in the active fracture model that's done as an exponent effect of saturation?

HOUSEWORTH: Correct.

VAN GENUCHTEN: Which seems to be working well and actually these figures are from a paper by Leo, et al, and I happened to go through that before the meeting. So, the flow data initially matched quite nicely, the multi-transport data. And, then, of course, you guys point out that matrix diffusion somehow has a problem, and then I think one of the things was to kind of artificially increase the contact area between fractures and matrix; right?

HOUSEWORTH: That's correct.

VAN GENUCHTEN: Or, I don't know if that goes into the tortuosity factor here. That may be another thing.

HOUSEWORTH: Well, yeah, if you have that paper, you'll see that there was an additional fit with an even treater enhancement of fracture/matrix interaction. It goes beyond what you would normally--tortuosity, you don't go over 1, but this would actually, if you just put an end to the tortuosity, would drive you to a factor higher than one. But, there's other things that influence fracture/matrix interaction other than just the tortuosity.

VAN GENUCHTEN: I want to go back to the discussion this morning about hydraulic contact between fractures and matrix. And, this is something that I always believed in, and my feeling is that this is where it's again also testing it for fractures, is where we know that there is very little contact sometimes with what we call Q-tens, or these clay deposits on aggregates, and there is a very, very slow contact between the macropores and the micropores. In fact, I was born in Holland. They still find little aggregates that have sea water type soil composition, you know, after several hundred years.

So, in this case, if there is a saturated conductivity, permeability problem between the fractures and matrix, then you still can, without going to the active fracture formulation as being used in this paper, you can explain this lack of interactions between the fractures and matrix by a lower conductivity of the coatings of the skin.

This also would then not necessarily, because of this, you don't necessarily have to go to a larger area for matrix diffusion, because matrix diffusion, soil diffusion will be less effective by your porosity than fluid flow. I think this will be, I don't know, I'd like to have your feedback or maybe of some of the others, but I think this is something that is worth investigating. The basic philosophy will be the same, except some of the physical processes will be slightly different in terms of implementing a model like that.

HOUSEWORTH: Well, it's clear that there's a number of factors involved in this fracture/matrix interaction. There's the hydraulic conductivity of the connection between them, as you point out, maybe inhibited by calcite coatings. There's the diffusion coefficient itself, and the effects of tortuosity. There's the flow focusing, the geometry of the flow, and all of these things are kind of put together into this one kind of description, and all the details of what various factors are causing the effect are not necessarily known.

So, yes, I mean I agree that there could be some additional investigation. One of the important sensitivities that we would like to run, we have a planned experiment with a block of fractured rock from the ESF, and with that block, it would be possible to look at more directly the relationship between flow and the active fracture parameter, and transport and the active fracture parameter, and in fact the fracture flow behavior in fracture networks, where we could have a greater control over the system. And, so, we kind of look forward to that as providing some additional confidence for how we're treating this.

CERLING: Dan Bullen?

VAN GENUCHTEN: I have another question related to in this TSPA model, you use a Bucket type model for flow in basically the alluvium top; right?

HOUSEWORTH: In the infiltration model, yes.

VAN GENUCHTEN: Yes, right. Have you tested that against the vitreous equation, a more complete description?

HOUSEWORTH: No, I don't believe we have. Alan Flint is here, and if he would like to comment on that?

FLINT: Yes, Alan Flint. We have done some comparison between the Richard's equation and the Bucket model, and that's how we did our original calibrations, probably four or five different papers on the Richard's equation applications and infiltration values. When we developed the Bucket model application, we did it to try to match the results we saw, because we couldn't use the Richard's equation over the extent of Yucca Mountain.

So, for some limited cases, we did a fairly good job matching, but we have some other issues we'd like to have gone back and redone that with more Richard's equation, and I'm actually working on a Richard's equation version now, and we may try to incorporate that into tuff at some point, take an infiltration model to do that.

So, we have done some and had good success with it. But, we haven't done as extensive as we'd like to.

BULLEN: Bullen, Board.

Could we go to Slide 32? This is the drift shadow. Basically, the effectiveness of the drift shadow is predicted by the modeling studies. Do you have any actual natural analogs or any real world scenarios in which the drift shadow has been observed, and in which you could support the claim that the radionuclides are predominantly in the matrix and as radionuclides enter a zone where there's no fracture fault, do you have an example of where a drift shadow actually exists in nature?

HOUSEWORTH: I'd have to say at this point we don't have any supporting data for that. I'd point out, though, that what we're utilizing in terms of the PA models that are going forward is simply that some radionuclides enter the matrix or the fractures, depending on the conditions of water flow through the drift, and the conditions of undisturbed flow beneath the drift. And, it seems like a reasonable way to treat it. But, as far as kind of real world data to support drift shadow effect, we're still basically looking for that.

BULLEN: Bullen, Board.

Then, can we go to Slide 34? This is sort of a suite of transport times for tracers. And, I guess the first question I have is which of these curves would best represent the type of curve that Russ Dyer showed us just before your presentation?

HOUSEWORTH: Well, our base case model, like for technetium, would be here, this mean present day climate curve.

BULLEN: Okay. And, that would be basically the UZ transport for technetium basically from the release point to the top of the saturated zone? Or is that all the way out to the--

HOUSEWORTH: No, no, that's just to the saturated, the water table; right.

BULLEN: To the water table. Okay. Then, I guess the follow-on question for all this family of curves is if the drift shadow effect isn't as prevalent as you expect, how would you expect these curves to change? What kind of results would you expect to see?

HOUSEWORTH: Well, this one has only fracture release.

BULLEN: Okay. So, that's the worst case scenario for if the drift shadow doesn't exist, it will look like that?

HOUSEWORTH: Right.

BULLEN: Okay, thank you.

CERLING: Frank?

SCHWARTZ: Yes, Schwartz.

Jim, as I was looking at your presentation, I sort of noticed that you seemed to accentuate lateral diversion, that it seemed that your lateral diversion emphasis was, say, stronger than Alan's this morning. I wonder, I mean, is that just the way the model comes out? I mean, how do you sort of reconcile the two sets of--

HOUSEWORTH: Well, I think the thing that was driving our model towards the inclusion of lateral diversion was the chloride data. And, it seemed to be better fit by the model with lateral diversion. I think it's a relatively weak effect, and like I said, it's not the old conceptual model diversion where nothing is getting through, and virtually everything is diverted into faults. This is more of a smearing out of infiltration patterns over the block. And, it seems to be somewhat more consistent with the chloride data.

CERLING: Ron Latanision?

LATANISION: Latanision, Board.

We've been talking about analogs to a certain extent, and I continue to be impressed by the analogs that appear in geology and the analogs that appear in solid state chemistry, and once again, there's another. I'd like to turn to the breakthrough curve that Russ Dyer showed in his presentation. That sort of data is very, very similar to the kinds of data that would be collected if one were interested, for example, in studying the transport of hydrogen through metals, which is of relevance if you're interested in the phenomenon known as hydrogen embrittlement of metals, which has occupied a lot of my research attention over the years.

These trenches can be used to determine such things as effective diffusion coefficients, or in this case, effective permeabilities, perhaps, and also equilibrium concentrations of solute, like hydrogen. And, so, my first point is I think you could actually mine these kinds of data for information that I haven't, and maybe you have done this, but I think you can determine such things as effective transport characteristics, dissusivities. On that basis, what is typically done is used the half rise time as a means of deconvoluting this data to get to an effective diffusion coefficient.

So, on this basis, I would interpret those data to show that the effective diffusion coefficient of water in this system is actually faster for the solid curve, which is the saturated zone, than it would be for the unsaturated zone, which makes some sense, I mean just based on the location of the half rise time, and the deconvolution of this data.

It's also interesting to me that in treating this data, those two curves have been added, and I'm just curious to know why they've been added. I mean, one possible way of interpreting that, and maybe I'm answering your question, since I've asked it, I'll go ahead and do it, but if you were to take the position that in order to achieve the consequence that was of interest to you, for example, in hydrogen embrittlement, you're less interested in the breakthrough time than you are in the time required to reach a level of concentration of hydrogen that causes embrittlement in a given metal, and the concentration level will be different in different systems.

So, for example, you could argue here that if you were adding--you might argue the case for adding these two together by saying that perhaps there is some level of water which is being transported through the saturated zone to the repository level, and another distribution of water being transported through the unsaturated zone to the repository level, and when those two accumulate at the repository level, you may achieve some level of concentration that is of consequence from the point of view of whatever, whatever phenomenon might be of interest.

I'm just wondering if that's the logic involved in that in these two together?

HOUSEWORTH: Well, actually, this isn't really a strict addition process here, at least for the combined curve. It's more of a convolution of what's coming out of the unsaturated zone, and then that--into the saturated zone as a source term, which is a distributive source term over time. And, so, what you see is that one curve represents what happens when you put something into the saturated zone, but the combined curve allows for the time distribution of releases entering the saturated zone to affect the overall curve.

LATANISION: This is just water though?

HOUSEWORTH: Yes, in fact, this curve is a little different than what you see for the technetium curve. This one used a higher diffusion coefficient that was more like for tritiated water. Technetium has a somewhat lower diffusion rate.

LATANISION: Latanision, Board.

Let me ask what I just said a little differently. Is it your opinion that the transport of water through the saturated zone is faster than it is through the unsaturated zone? Is that a conclusion that you would--

HOUSEWORTH: Yes.

LATANISION: You would?

HOUSEWORTH: Yes.

LATANISION: And, you're comfortable on the basis of this data, or other data?

HOUSEWORTH: Well, this isn't data. This is a model.

LATANISION: I understand. If you had data.

HOUSEWORTH: Yes, and, of course, we don't have a lot of data at the mountain scale that we've been able to utilize. It's kind of inferred from things like the isotope signals that we've been able to measure, you know, other evidences that are more indirect. We haven't had the opportunity, nor do we have the time, to put in the tracer at the repository level and see, you know, how fast it comes out at the water table. So, anyway, this is strictly a calculation.

LATANISION: Okay, thank you.

CERLING: And, I think we're running about 15 minutes behind time, or so, and I will reconvene at--in ten minutes, so 3:55.

(Whereupon, a brief recess was taken.)

CERLING: Our next speaker is George Moridis from Lawrence Berkeley National Labs.

MORIDIS: Good afternoon.

There's a whole host of processes that we will try to discuss. We'll discuss the radioactive species and transport processes, the model validation and confidence building, using various tests, field tests of various scales. Mountain-scale solute transport studies, including radionuclides with different sorption affility to the host rock, the different climatic regimes, as well as different levels within each regime, also different ways to release the radionuclide, both instantaneous and continuous release.

We will discuss colloids. Our discussion will focus on four different colloidal sizes, and different filtration behaviors, and we'll conclude with a discussion of uncertainties, as well as conclusions and comments.

It's important to note from the beginning that transport is not in itself, standing by itself, is not a self-supporting type of study. We draw extensively upon a number of other areas that have been researched and have been already presented here earlier today.

For example, I can show you over here, that we rely very much on climate and infiltration, degradation, very much on the saturated zone flow. Actually, we will come back and discuss this issue a little bit more. Engineered barriers, the radionuclide, the colloid transport, the radionuclide releases.

In a sense, I'd like to point out that in the whole chain of the transport processes, or the processes that affect transport at Yucca Mountain, we're near the bottom of the chain. In that respect, all the uncertainties that exist in the outer processes cascade, propagate through the system into the issue of transport.

And, that is extremely important, especially in the case of hydrogeology, which is the dominant factor affecting transport. In essence, perhaps I may be excused if I use the expression that the performance, the transport performance of the whole system, the UZ system, arises and falls with the unsaturated zone flow system.

You have seen quite a few depictions of the subsurface at Yucca Mountain. I'll show you this one here just to help point out a couple of important things in the ensuing discussion. First of all, this is the, in terms of the position of the repository, it is located TSw, mostly TSw. Below the TSw, which is the Topopah Spring, there is the, in the northern part, there is the Calico Hills z, the zeolitic, which is characterized with extremely low permeabilities in the matrix, and the fracture permeabilities, much, much larger than that in the matrix.

In the southern part, we have the vitric Calico Hills, which is characterized with rock impermeabilities in the matrix and in the fractures. The importance, again, of hydrogeology in the issue of transport cannot be over-emphasized, as you will see in the following discussion.

The processes we are discussing are the following. Advection, this affects both solutes and colloids. Matrix diffusion. We have quite a bit of this. This can occur in the unsaturated zone in the fractures, or in the presence of perched water bodies, and also in the matrix. Dispersion, which we're finding plays rather a minor role. In the case of solutes, we have sorption. In the case of colloids, we have a couple of mechanisms. One is pore size exclusion, which is mechanical straining, and also from filtration and attachment, which is a physical chemical process, and radioactive decay, which, of course, affects all radionuclides.

The radioactive species that we are discussing today in terms of solutes include species that have various Kd's, various sorption of native rocks, from non-sorbing, to very strongly sorbing. In the case of colloids, I'll just show you three classes. The first class is consists of different kinds. The one is a true colloid, in essence, colloids from supersaturation, and also waste from colloids, which are formed from radioactive substances. The important thing about Class I colloids is that, in essence, the whole colloid is radioactive.

Then, we have Class II and Class III colloids. In Class II colloids, we have the native colloid, for example, native oxide or clay, into which the radioactive isotope has been sorbed irreversibly. By this, I mean it's become part of the structure. And, in Class III, the sorption is reversible in the sense that it's on the other surface of the system, and can be exchanged with environment.

In the process of validation or confidence building, we had much model, with field tests, which covered various scales. The first one, which is Test 1, and Jim Houseworth already presented to you the information about this test at Busted Butte. We matched the fluorescent plume at Busted Butte. This is Test 1-A. And, also, we matched the concentration of bromide that was also injected.

What we see is that what we saw in this effort is that the comparison between predictions of field data was quite good. The next scale, which is a millimeter scale, involved the comparison between field data and numerical predictions for the Test 1-B, again at Busted Butte. And, here, this scale is about, as I said, about 1 meter, and the comparison between predictions and observations is quite good.

Moving up the scale, the left scale, in Test 2-C, always at Busted Butte, the scale is 2 to 3 meters, and when we compare the concentrations of both bromide and lithium, we do see a pretty good agreement between observations and field data.

And, the largest scale that we had available for this type of confidence building was the Alcove 8, Niche 3 test, where the scale is about 20 to 30 meters. In this particular case, the ability to match observations and predictions, with the use of the active fracture/matrix model, and what you can see is that we can get a pretty good match between the two.

Now, I would move in the discussion of the 3-D mountain scale transport studies. I'd like to highlight the objectives of this study, because I want to avoid misunderstandings regarding the following results. The objectives of this work was to stress the system under impossibly aggressive, possibly attempt to use impossibly conservative conditions, in an effort to determine the main pathways of potential radionuclide transport to the water table; identify the dominant processes which affect the transport and retardation; evaluate the relative importance of processes and phenomena; and, finally, determine the relative transport behavior of general types of species, solutes versus colloids, nonsorbing versus sorbing. In essence, the focus is on the relative performance, not on the actual prediction.

If I can use an analogy, it is roughly analogous to over-inflating a tire suspected of leaking, and submerging it under water to see where the leak is coming from. It's exactly what we did. We over stressed the system trying to find the weak leaks, the main pathways, the early pathways of transport. Again, as I said earlier, it's not an attempt to predict travel times to water tables under any plausible release scenario.

I said that we have a conservative. What do I mean by this? Well, there is a sequence of very conservative approaches with that. First of all, would not consider drip shields, and we assumed that whenever a drop of water falls from the ceiling of the drift, it flows down through the canisters. That's a pretty serious assumption. As long as water does not come into contact with the radionuclides, we do not have a transport problem, period.

So, as long as there are drip shields, effective drip shields, or as long as there is a canister that's not being compromised, then we don't have a transport problem.

By the way, each one of those cannot--to hundreds of thousands of years in terms of delay in the onset of release.

All the radioactive packages in the entire repository, I mean, the whole footprint, are assume to rupture simultaneously. The radionuclides are released directly into the fractures, and we do not consider retardation effective of the invert or the invert which has porous media properties, or actually we don't consider anything like an artificial barrier, which can be maybe present.

The effects of the shadow zone are ignored in this study. The vertical fractures are open and continuous throughout the UZ top to bottom, all the way through the repository. There is no retardation either for solute sorption or colloid attachment in the fracture walls. So, the fractures are assumed to be open. They do not sorb, and colloids do not attach there. We do not account for sorption or attachment, properties of fracture minerals, which we know to be considerable.

The horizontal fractures are modeled as interconnected, and they're also connected, directly or indirectly, with the vertical fractures. The distribution coefficients were estimated over longer concentration intervals, I mean, this is an approach which results in milder Kd's, which is even more conservative. We do not consider any potential chemical stabilization of soils, for example, through precipitation. We do not consider the issue of colloid stability, which is, you know, anything but assured, especially near the release points. There's all kinds of chemicals, thermal processes that can easily stabilize the colloids. It can delay their onset, their appearance in the fractures by thousands, tens of thousands, or even more, for years.

So, it's important also to indicate that in all of this work, we are fairly perched on the shoulders of the existing hydrogeologic mortal. So, whatever certainties there are, they are immediately transmitted in the transport model.

Starting with technetium. Technetium has the rather unpleasant behavior of not being sorbing. In this particular case, we are assuming sometimes release. And, by this, what I mean is that we put a mass throughout the repository footprint. And, the interesting thing to see here is the effect of various climatic regimes on the breakthrough curves.

What we see on the left is, of course, some of the mass that has caused the bottom bound area, has got to the water table. For present day infiltration condition, and keeping in mind that the important thing is the relative performance, is that for mean present day, we have an arrival, relative arrival, at about 100 years. If we have the lower and upper limits of the present day infiltration, then transport can--arrival of 10 per cent of the radionuclide, which is a good sign.

Actually, I'd like to step back and explain that what I usually use is two numbers. One is T-10, which is the time it takes for 10 per cent to cross the bottom bound area, and this is an indicator of the fast arrivals. And, then T-50, which is the time for 50 per cent to cross the bound area. And, that's an indicator of the average overall performance.

In terms of fast arrivals, we see that when the upper, the present day climate is assumed, we have the reduction in the time for 10 per cent of the mass to arrive at the water table, by about an order of magnitude. However, if we assume that we have the drier present day climate, then the arrival goes from about 100 years to 10,000 years.

So, the important thing to see here is the direct effect that infiltration, the climatic regime has on transport. We see the same thing in the assumption of infiltration, and glacial infiltration, both of which are far more wetter, far wetter than the present day infiltration.

It was very interesting to us, or important to us, to find out the transport patterns of technetium. So, we looked at two particular places. One is at the bottom of the TSwu, which is the hydrogeologic unit where the repository is located, and the other is right immediately above the water table.

As early as 10 years, looking at the bottom of TSw, we're beginning to see some, very low, concentration of appearance of technetium. This is in the fractures. Keep in mind that what I'm sure is relative concentration, so these results translate directly to things like concentration, or dosage, or whatever.

In the matrix, we see a somewhat different picture, actually, a vastly different picture. Here, we see that we're beginning to see things of much, much lower concentration in the southern part of the proposed repository, and the reason is that here, there is a permeability between matrix and fractures, so this is the reason why we see things as far as fracture is concerned, the north is where we have the dominant fracture flow, so we do see stronger, we see the presence of radionuclide only in the north.

At a hundred years, we're beginning to see a somewhat different, things are beginning to become more interesting. Looking at the distribution of the concentration in the fractures on the left side of this viewgraph, we are, in essence describing the presence of the faults, the distribution of the radionuclides here, in essence, coincides entirely with the two faults, this is the Drillhole Wash Fault, this is the Pagany Fault. And, here, we're beginning to see the appearance of another fault. This is at 100 years at the bottom of the TSw.

Conversely, near the matrix, we are seeing that the concentrations are in the southern part. Again, the reason is because here, we do have matrix flow.

What is even more interesting is what's happening at the water table level. As early as ten years, we can easily outline the three major faults over here, the Pagany Wash Fault, the Drillhole Wash Fault, and I forget what this one here is, and the appearance of the presence in this place here, which also identifies another fault.

In terms of matrix concentrations, the thing we see at ten years is that we're seeing some faint signature over here of the glacier, but this corresponds to the fact of the main faults. In essence, what we're seeing is that the water table, assuming the validity of the hydrogeologic model, transports the presence in the matrix is through the fractures, in essence, as the radionuclides come down, they get into the matrix only through the fractures of the fault over here.

This becomes even clearer in the case of 100 years, and we do see here very clearly the signature of the faults. We look at the concentration of the percolation at 100 years in the matrix, in the fractures, and we can identify the faults. And, the interesting thing again, is that at the water table, unlike at the--the concentrations in the matrix follow very closely those in the fractures, which indicates that the main transport conduit in this case to the water table are the faults, which is not inconsistent at all with the previous discussions.

How does this correlate to the deep percolation? Well, the relationship is one to one. There is direct correlation of water flow to the UZ. On the left, you see the infiltration or the deep percolation at the repository level, and here, the water table. If we compare the patterns, the transport patterns, and the flux of the water fluxes, we see that the correlation is direct.

In essence, that's a sharp reminder again that whatever certainties exist in our hydrogeologic model, they can start automatically, undiluted, into the transport model.

Moving to neptunium for a second. The main difference between neptunium 237 and technetium 99 is sorption. The main difference in terms of behavior between the two is the fact that this one here is a mild sorber. It doesn't sorb very strongly. But, even so, this is sufficient to increase D-10, again, the time it takes for 10 per cent of the released master course at the bottom bound area, it sufficient, you know, this mild sorption, to increase it by about an order of magnitude. And, this is persistent in all the cases, different infiltration scenarios, and also different levels within the infiltration scenario.

So, what we're seeing here is the effect of sorption, and this is the second important retardation mechanism in the case of radionuclide transport.

As far as the transport pattern, we see the exact same thing we saw earlier. Again, at the bottom of the DSw, we see that in the fractures, the main transport conduit is the faults, whereas, in the matrix concentration indicates the matrix flow in the southern part where we have a sufficiently high matrix permeability.

And, we see the same thing actually at 100 years at the water table. We see the exact same thing as before at ten years, we can take a look at the concentration, distribution of the neptunium, you can identify the faults, and, again, we don't see any matrix flow, evidence of matrix flow. The matrix concentrations here indicate that the source is the radionuclides, that they'll arrive in through the fractures. And, this is even stronger at 100 years.

Moving to a really strong sorber, such as plutonium

239, plutonium, here, we see a different picture. This is sufficiently strong that in quite a few cases, not even 10 per cent of the radionuclide ever reaches the water table. Of course, we have seen the same bottom as before. The wetter the climate is, or the higher the infiltration level is, the more radionuclide arrives with the water table.

By the way, plutonium here is indicative of a whole class of very strong sorbers, and it's actually the one with the lowest sorption among the class of the strong sorbers. So, in that respect, the system appears to be a pretty good barrier to plutonium transport.

Up to now, we've been discussing instantaneous release. Now, we're looking at continuous release. In essence, we have radionuclides being released continuously throughout the whole footprint of the repository. Now, we cannot compare masses. We compare fluxes, because the mass keeps increasing, you keep adding more and more mass to the system, so we compare the flux at the bottom of the repository versus that at the water table.

And, again, the important thing to see is the relative behavior of technetium versus neptunium versus plutonium. As before, from technetium to neptunium, which by the way the fall roughly in no more than the sorption, neptunium being a mild sorber, we have an increase in the T-10 by an order of magnitude. What looks quite good, looks apparently good, but may not be so, is the plutonium, which shows extremely low arrivals at the water table. However, one needs to look into the system a little bit further, because the problem with radionuclides is, of course, with daughters, what the daughters do.

In the case of plutonium, we look here at the relative mass fractures of the release point, and what we see is after about roughly 100,000 years, we don't have any plutonium being released, because the source has decayed into uranium 235. What's very, very interesting, though, is at the water table, if we compare the mass fractions of the radioactivity arriving, we see that it only takes about 10,000 years, and practically everything is uranium 235.

Now, this is pretty much what's happening to the relative masses. Now, it's not how much is arriving down there, and for this, we go to the third figure over here, and you see that we have very slow arrivals at this point. I mean, very low arrivals. But, after about 10,000 years, we have very large arrivals. The reason is two-fold. Uranium 235 has a much higher half life, a much longer half life, about 100 million years, and the other problem it has is it's a mild sorber, as opposed to plutonium 239, which is a pretty strong sorber.

So, in essence, this is shown over here to indicate the importance of the need to account for daughters in the study of change.

Moving to colloids now. We considered four colloids of different sizes. We give the products of plutonium dioxide, and what we're looking at over here is just mean present day climate. In the left, what is termed Case 1 is the case of very slow declogging, in essence, filtration is a--straining is a mechanical process, in essence, the colloid is too large to get through the force. The clogging or filtration is the physical chemical process, and it's a kinetic process, and here, we assume we have a slow declogging process.

Here, we have a fast declogging process. So, in essence, they are attached, and it takes a long time for them to be detached in here, and then they are detached relatively earlier.

The very interesting thing is that relative to the radionuclides, the very, very early arrivals of colloids, in the case of larger colloids, smaller colloids appear to be very effectively dotting by the system. The reason is that they are sufficiently small for them to be able to diffuse into the matrix. However, the larger the colloids, the earlier the arrival. I mean, there are three reasons for that. Number one, the larger it is, the small diffusion coefficient, so it becomes harder to diffuse into the matrix. Second, the larger it is, it has mechanical problems in getting to the matrix, because it's too large to get into the pores. The third reason is that when a larger colloid becomes confined more and more toward the center of the fracture where the velocity is about 50 per cent higher than the average water velocity, so they travel faster.

So, we see this consistently in both the case of the fast declogging and slow declogging. So, the important observation from this is the effect basically of colloid size and transport.

In terms of fractures, they're kind of interesting to me, too. If we use a 6 centimeter colloid and we look at 1,000 years, again, the distribution in the fractures indicates, clearly identifies the major faults that occur at the site. If we look at the matrix distribution, we see that that, too, follows the fractures. In essence, the pathway to the matrix is through the fractures. The colloids move down through the fractures because they're sufficiently small, they can get through the matrix.

We see a different pattern in the case of the larger colloid, the 450 nanometer colloid, at the same time, a thousand years. In essence, what we see here, that every fracture, not just the faults, is a conduit here. The reason is the fact that there's very little retardation in the fractures, number one. Number two, they cannot get to the matrix. So, that's why we see all the fractures here transmitting. And, when we look at the matrix, the highest concentration is not to the north, because, again, they cannot get through the matrix, but there is some, although quite small, actually very small, matrix flow.

We have discussed, directly or indirectly, uncertainties up to now. The most important uncertainty, of course, is that in the hydrogeological model, and also the uncertainty in the infiltration. And, we've seen how this affects our predictions.

We also looked at some uncertainties that can affect some other issues. So, what we see here is the effect in the diffusion coefficients, how easily the radionuclides can diffuse into the matrix. What we did was we arrange the diffusion coefficient up and down an order of magnitude, and actually on the upper part, we gave it the diffusion coefficient of the chloride ion, and trying to see what kind of effect it has. Roughly speaking, we get, by doing this, we get about plus or minus less than an order of magnitude change in terms of T-10 or T-50. This is both the case of the technetium and the neptunium.

In the case of plutonium, because it's such a strong sorber, we have a different picture there. We do have early arrivals, but the quantities are much, much, much smaller.

In the case of uncertainty of the sorption coefficient, we'll first focus on the middle one over here. This is neptunium. We're not looking to technetium because we already know it's non-sorbing. In the case of plutonium, it's such a strong sorber that the sorption coefficient did not have very much of an effect.

What we did here was the following. We used the highest and lowest values that were measured in laboratory experiments from Yucca Mountain rocks, and based on this, we see the uncertainties there, and we covered the whole range, can probably change the T-10 or T-50 by about an order of magnitude.

However, the interesting thing was when we tried to find out what is important in terms of geologic formation in transport retardation, one part of the horizon of the geologic profile is the one that's really most effective in providing retardation.

So, what we did was we lost some relations by setting the Kd's to zero for the three main rocks, the TSw, the CHz and CHv. And, what we found were, at least to me it was pretty much of a surprise, was the TSw seems to be the main culprit. TSw seems to be the unit, the rock, that provides the lion's share of retardation. We see this in the case of neptunium here, and we see this even stronger in the case of plutonium.

CHz seems to have the least effect, while it's to be expected, because most of the flow goes to the fractures, where we don't have an absorption, at least in our assumptions, and CHz has some effect, but, it's minimal compared to that of the TSw.

The uncertainties, of course the issue at the fracture matrix, Jim has already touched on this, so I will not expand on the subject.

A very interesting thing to me was, in trying to figure out why we have these relatively early arrivals, so, one of the assumptions was that, well, we do this because we have releases throughout the repository footprint, including the gridlocks that include the fault. So, we run an additional set of simulations where we did not release directly to the faults, and we did not release in the gridlocks that straddled the fault. So, in essence, we created a kind of three cell plan that followed the faults, where we did not release anything.

The interesting thing is that at the bottom of the TSw, we did see quite a bit of difference, however, when we saw arrivals at the water table, as described here, by the breakthrough curves, the effect was minimal. In essence, that seems to indicate that there is enough lateral flow, a lateral conductivity of the fractures, or possibly the issue of lateral diversion, that, in essence, by the time we get to the water table, the effect of not releasing directly into the faults is more or less completely circumvented. And, that was consistent in the case of what is the times releases. We tried that before, we tried technetium, neptunium, uranium 235 and plutonium, and we get the same consistent picture.

So, I'm arriving at the end of this presentation, and I'd like to reiterate the extremely conservative approach we took on this one here. This is almost impossibly aggressive approach in starting this subject. However, I'd like to reiterate once more the importance of very significant uncertainties we have in both the flow and model, our hydrogeologic model, as well as the aspects I've already discussed. And, these can change the picture drastically, because the transport model, there is also, I showed you, if you rely directly on the hydrogeologic model.

In conclusion, we do see the radionuclide transported, dominated and controlled by the faults, which provide fast pathways for downward migration to the water table, used in the current hydrogeologic model always. But, those flow patterns follow the infiltration, percolation and distributions, and the relationship is one to one.

There is direct relationship between increased infiltration, water climatic regime, and shorter arrival times at the repository. Radionuclides move faster and reach the water table earlier, which is characterized by the presence of highly zeolitic CHz layers, as well, of course, as the faults.

The highly conductive Drillhole Wash and Pagany Wash Faults are the main pathways of transport in the northern part of the repository. Diffusion into the rock matrix is the only mechanism for non-sorbing solutes. Mechanical dispersion is expected to be minimal.

Hydrogeology is the most important factor affecting transport. I cannot over emphasize that. Sorption and matrix diffusion are the main retardation processes in the transport of sorbing radionuclides.

The unsaturated zone of Yucca Mountain appears to be an effective barrier to the transport of strongly sorbing radionuclides. We discussed plutonium 239, but it also applies, actually even stronger, in the case of strontium, radon, thorium and the recent protactinium.

Under the conditions of this study, the effectiveness of the unsaturated zone of Yucca Mountain as a natural barrier decreases with a lower sorption affinity of the radioactive solutes, and longer half lives. In evaluating the barrier efficiency, the entire radioactive chain must be considered.

And, finally, under the conditions of this study, the unsaturated zone of Yucca Mountain appears to be an effective barrier to the transport of small colloids.

However, the barrier effectiveness decreases very rapidly with an increase in colloid size.

With this, I'd like to conclude my presentation. If you have any questions, I'll be delighted to answer them. But, please be gentle.

CERLING: Priscilla?

NELSON: Thank you. Nelson, Board.

I liked the consideration of the daughters, that was good and well presented. I have a question, just off the top, though, I mean, you modeled Drillhole Wash as highly conductive, and then it shows up as highly conductive, so, the question becomes how do you know it's highly conductive.

MORIDIS: This is a great question, which must be addressed by the hydrogeologist in charge of the hydrogeologic model. I'm the consumer of this information.

Actually, let me suggest something. This is a very important question, and although I'm co-presenter and familiar with the subject, I'm not at the level that is commensurate with its importance. May I ask that Bo Bodvarsson, who is intimately familiar with this, answer this question? Bo?

NELSON: He may be too shy to come up.

BODVARSSON: Priscilla, you always make me blush.

How do we know that they are (inaudible)? We don't know for sure that they are, because we have done it only on a limited amount of testing. But, some of the indications like from Jim Paces, results that show that there is a lot of calcite in some of these washes seems to indicate that there is a lot of water flowing, and seems to agree with what George just said. But, we don't know that for sure.

NELSON: Thank you, Bo.

This seems to be, what I take from your study is the paramount importance of this particular assumption in how the mountain is working, and, therefore, I know the Board said this before, and many people on the Board have said this before, but it seems important enough to actually do some work determining directly permeability of faults.

Thanks.

CERLING: I think Dan was next.

BULLEN: Bullen, Board.

Could you go to Slide 11? Actually, I was very interested in the data that were shown in first the original interface area prediction, and then the increased interfaced area of prediction for the confidence building in the transport here. Could you explain to me, I mean I understand how you can modify the parameters to fit the data, can you explain to me the justification for the original prediction, and then why the parameter had to be modified?

MORIDIS: Well, I can explain why the area has to be increased. In the case of flow, which is the primary reason why the shift, the active matrix fracture model is developed, there is a trigger, and that is there is an irreducible, beyond which we cannot move. However, in the case of transport for diffusion, the only thing that needs to be there is a continuous wet face. As long as it's wet, it will, regardless if it's reducible or not, you know, (inaudible) and moisture will occur. So, it makes sense why we need to increase the size for that.

BULLEN: Thank you. Bullen, Board. One more quick question on Slide 13. And, actually, it's not a question. It's more of a comment. I wanted to compliment you on the very explicit explanation of the conservative approach. You, in my estimate, effectively moved any masking effect of any other calculation you would do, and then you got to the point of I can take a look at the parameter, I can look at the transport, and I can under the phenomenon without having to worry about whether I had drip shields, or whether I had intact waste packages, or if I had any other types of flow in the matrix. And, so, I want to compliment you on this, because it made the presentation that followed very clear.

MORIDIS: Thank you very much. I have to tell you flatly, it never hurt me.

CERLING: Ron?

LATANISION: Latanision, Board. And, I have to add that that's a rare compliment from Dr. Bullen.

Throughout your talk, you used language that refers to, and this is in the discussion of the breakthrough transients, shorter arrival time at the repository. Well, wait, hold on. What is actually the most important criteria? Is it the arrival time at the repository, or is it some measure of the dose which is a consequence?

MORIDIS: In this particular case, because I used relative concentrations, I mean, as long as you know what is being released at the top, then what you get at the bottom, I mean, it's relative. In essence, it's direct. Okay? What you see, it's not masking anything. It is the actual dosage, or whatever, just multiplied by whatever is released at the top. However, I'd like to reiterate the fact that what's more important in this presentation is not the arrival times, which are used for lack of a better term, it's the relative magnitude of the quote, unquote arrival times, sorbing versus non-sorbing, colloid versus solute, in this particular case.

If somebody puts a gun on one's head and says, well, my head, and says, well, what does this represent, I could say that this is the possible, and possibly, actually, conservative approach that would define, without a doubt, the lower part of the envelope, the lower solution. So, that's why I feel confident we state in there for strong absorbing radionuclide, this is an effective system, under these absurdly, insanely conservative conditions, we still get a very good retardation, plutonium and that type of thing.

But, the important thing is how they compare to each other, because even if things, because of a moralistic description, which is what this does, even if things appear to have different actual arrival times, or prediction level times, the relative sizes I think will persist. The relative marketers will persist regardless of what the absolute is going to be. That's the important thing.

LATANISION: Latanision, Board.

No, I will buy that. I think you're right. I'm simply making the point that in a really pragmatic sense, what you're interested in is some measure of the dose, or tolerance that the system allows you.

MORIDIS: The results transmit directly into dose. You just multiply this by the release, and you get the dose. It's relative.

LATANISION: I would just suggest making that statement conceptually, so that it's clear that you're measuring relative parametrics, but on the other hand, the ultimate point is related to something like the dose.

Thank you.

CERLING: Frank Schwartz?

SCHWARTZ: Yes, Schwartz.

George, you released the entire inventory. Actually, what proportion of that whole inventory turned up at the bottom of the fault at the water table?

MORIDIS: You mean what crossed the bottom boundary; right? In some cases, all of it. You know, in the case of the technetium, all of it. And, you just look at the times, and you figure out how much, I mean, what can show up.

SCHWARTZ: Well, I guess I was thinking of the early arrival, you know, the hundred year time frame.

MORIDIS: You look at the fracture. I mean, can I go to 14, please? Okay, on the left side is the fracture, the mass fracture. This is a very regular breakthrough curve. The fracture has crossed the bottom boundary. So, in essence, for 10 per cent or 20 per cent or 50 per cent, you just can get it straight from the curve.

SCHWARTZ: I guess I was thinking of your, for example, your red figures where you could see the outline of a fracture vaguely represented there.

MORIDIS: This is different because this is a cumulative effect, and over there, it's a snapshot in time, what happens on this particular time. In essence, if we degrade--we get to that. It's not--

SCHWARTZ: Yes, it's Number 16.

MORIDIS: So, at the hundred years, roughly about 10 per cent, I mean, we saw from the breakthrough curve, about 10 per cent has crossed the bottom boundary.

SCHWARTZ: I guess the second question I had was how was that proportion of the inventory able to find that fracture? Because, I guess, you know, some of the conceptual models are that these fast pathways, the fractures in particular may be fast, but they're not carrying a large proportion of the water, yet it seems like a large proportion of the mass turns up here.

MORIDIS: This is an excellent question. But, the only answer I can give you is this is inexorably tied to the hydrologic model that we have. Based on this, in essence, I can see that the hydrologic model has lots of lateral connectivity of the fractures. So, in essence, it appears that these faults drain in a much larger area than the footprint, which is pretty small. Based on this model, the hydrologic model, which appears to be the best we have right now, this appears to be the case. Actually, these are washes, so, in essence, these are drainage basins. It makes sense that they would drain in a larger area of the footprint. I don't want to perjure myself. I'm not so intricately familiar with the flow model, but this appears to be the case using these tracers.

VAN GENUCHTEN: I have a couple of questions about your colloid parts. Have you done some sensitivity analysis, and especially actually in Slide 25, if you were to exclude colloid transport, did you try to--yes, 28--25 is just fine.

MORIDIS: This is not colloid. This is plutonium.

VAN GENUCHTEN: Did you include colloid facilitative transport with the plutonium?

MORIDIS: No. The reason is simple. This applies to Class I and Class II, in essence, either 100 per cent radioactive colloids, or irreversibly sorbed colloids. The problem, when we get into colloid facilitated transport, is the following. That we don't have a pretty good handle of what the natural colloids, oxides plus clays, are going to be. That's one uncertainty. And, in addition to this, the problem is that although we've been able to use linearized equations up to now, so we can have relative concentrations, there, we have, in essence, the product of two concentrations.

So, the problem is not only linear. We can solve it, but it's all functional what we put there, and we don't have substantially reliable data about natural colloids or even the concentration of the soils that might be of the radioactive particles that will be sorbed into the colloids, because we have two uncertainties, and we cannot linearize it. It would be unwise to use that like that.

VAN GENUCHTEN: So, could you hypothesize how the colloids might affect especially the plutonium curves?

MORIDIS: Easily. Okay, anything that you see over here, what you see over here is the colloidal particle going down. Okay? And, the green curve over there, that describes basically the decay at the source. But, after about a thousand years, or even 10,000 years, this will be about the same thing as a clay particle coming down the fracture. There's absolutely no difference. The behavior at this point where the half life has not really taken much of a toll is not, you know, it's roughly the same.

VAN GENUCHTEN: Now, your colloid transport, colloid facilitated, so you mention size exclusion.

MORIDIS: Yes.

VAN GENUCHTEN: So, you leave the colloids mostly in the fractures?

MORIDIS: Yes. The size exclusion varies with the various units, I mean, based on the particle or the pore size of the various units.

VAN GENUCHTEN: And, then, the other one you mentioned is filtration, attachment, detachment?

MORIDIS: Yes.

VAN GENUCHTEN: Do you run those together, or do you separate these?

MORIDIS: It's a kinetic equation, it has an attachment and detachment part. It's a kinetic filtration, the K+, K-.

VAN GENUCHTEN: Do you use the--

MORIDIS: I used the full kinetic model for this, because I'm not convinced that we can use an equilibrium model for colloids, not yet anyway. And, I have to tell you something else. You touched on the subject, which is, you know, a very sore point with me. We don't have any idea at all how the models that we have describe how well they describe colloid transport, especially in saturated media. We don't know what the kinetic parameters are, and we don't know how the way that we describe mathematically by using the product of tortuosity, all of these, we don't know how well these describe the system. The way we try to work out this by using the wide variations in the possible reported parameters, I use some of the data from Chris Ecopolis, who's come up with some attachment and detachment parameters.

VAN GENUCHTEN: You're sticking with this first kinetic--

MORIDIS: Yes.

VAN GENUCHTEN: Which may or may not be, you know--

MORIDIS: That's very possible, entirely possible. The only way I try to account my (inaudible) on the subject, is by varying tremendously the range. And, what this is, it doesn't make very much of a difference here. One is very fast attachment, the other is very slow, and we reference performance.

VAN GENUCHTEN: There's some alternative from relation, because this gave you an exponential distribution versus depth, especially when you start out with textural discontinuity. That's where the problems are.

MORIDIS: Right.

CERLING: Richard, the last question?

PARIZEK: Parizek, Board.

I have two daughters, and I'm not having any problem with them. How long do you have to keep your daughters in the house, more or less, before you have a problem in performance?

MORIDIS: My daughters live in Berkeley. Let me put it this way. I mentioned this, but probably did not give it enough emphasis. This assumes that the colloid somehow manages to be stable and gets in the fracture and starts moving. However, there are near field chemical thermophysical reasons for why this colloid cannot be stable for a very long time. Okay?

For example, if there is concrete somewhere near the release point, this is going to stabilize entirely and completely the colloid. There's going to be fluctuation. Or changes in pH, all of these things have not been accounted for. I just say all right, somehow colloids manage to escape. This onset may be a potential for hundreds of years. I don't know. I don't have this information yet. Okay? Once it manages to get there, and when we assume this very, very aggressive approach, then we see these relatively fast arrivals at the water table. But, I don't know what happens as it travels down, has it encountered chemical physical directions which further stabilize this, then it becomes far less of a problem. This, again, is a very, very conservative approach, especially for colloids.

PARIZEK: The idea that you release all the waste almost when you put it in, and that's not realistic, so the waste packages--

MORIDIS: Not only that. I mean, something very simple. Okay? I did not show this, but I have results where I put just 1 per cent of the fractures occupied by a matrix material, so the porosity is 100 per cent, of which 99 is air, and 1 per cent is matrix material. So, there is a very, very small minor partial fill, and this is sufficient to increase the arrival at the water table by an order of magnitude, 1 per cent.

Now, the fractures, we assume, are not clean. If we have anything like 30, 40, 50 per cent fill the fractures, this is delayed by four or five orders of magnitude.

PARIZEK: It's like you have an open elevator shaft.

MORIDIS: Exactly. Exactly.

PARIZEK: That's not probably the architectural character of fault zones.

MORIDIS: Exactly. And, there is no fill. There is nothing in the fractures. Okay? I mean, this alone is enough to push plutonium solutes by an order of magnitude in terms of arrival at the water table. I mean, increase the arrival times, okay? Again, this is very, very unrealistically conservative. TSPA has got me running far more in a realistic simulation. This is trying to find what's important. Where does the over inflated suspicious tire leak. That's the question we're trying to look to.

CERLING: I'm thinking in view of keeping a little bit close to schedule, thank you. We'll let you off the hook. And, the next talk is Bruce Robinson on the unsaturated zone radionuclide transport predictions and abstractions for total system performance assessment.

ROBINSON: Good afternoon. Or, I should say good evening. It's been a long day, and I hope to get you through the final presentation of this day. I'm going to be talking about the unsaturated zone abstraction model for UZ transport.

What I would like to do first, however, is to acknowledge my collaborators, who were instrumental in developing the model that I'm going to be presenting you today. Chunhong Li of Framatome; Jim Houseworth was involved from Lawrence Berkeley National Laboratory; from Los Alamos National Laboratory, Hari Viswanathan and Zora Dash and the late Peng Tseng; and TSPA modelers and analysts, Don Kalinich, Dave Sevougian, Barry Lester and Bryan Dunlap.

This is a summary of the topics I'm going to talk about today. What I want to do first is to go over the goals and requirements of our abstraction model for the unsaturated zone radionuclide transport. I think that will hopefully bring into fuller focus some of the things that have been talked about at various points today.

This model essentially integrates a lot of the work that's been presented today, and incorporates it into the total system performance assessment. And, so, therefore, the extent to which we're able to do that with fidelity to the original models is really key.

I'll then go into model formulation, how we are computing radionuclide transport through the unsaturated zone, show how this model is connected up to other parts of the total system performance assessment, TSPA submodels, both upstream and downstream of the UZ, get a little bit into validation of the abstraction model to prove that it's valid for the intended purpose, which is as the UZ component of the TSPA analyses. Then, I'll talk about some transport processes and parameters, and how they are represented in the model, and how their uncertainty of key parameters and processes is incorporated, and then I'll conclude.

First, I'm want to talk about the overall goals of a TSPA abstraction model for UZ transport. If you consider the problem of TSPA in terms of calculating a dose, and then work your way back to the UZ, that's what's depicted here. If we take, basically, our regulatory requirement is to look at how much mass of radionuclide is crossing the compliance boundary, and then we mix that in a given volume of water, 3000 acre feet of water, and that gives you a concentration.

So, in terms of calculating a concentration, which is directly related to dose, what we have is a radionuclide mass flux, M, crossing the compliance boundary, divided by a flow rate of 3000 acre feet per year, which we've set by regulation. So, what's really key here, is the arrival mass, radionuclide mass flux. That's what eventually will get to the compliance boundary, unless it decays or is retarded in either the UZ or the SZ.

So, what does that mean for the UZ transport model? Essentially, the UZ transport abstraction model needs to predict travel times of radionuclides, not necessarily concentrations, although as George showed, it's a very good diagnostic to be able to tell how the UZ models are behaving. Our real goal here is to predict travel times, rather than in situ concentrations in a plume or concentrations in a perched water zone or what happens when the UZ water mixes with the SZ water. Those are concentrations upstream of the final concentration which matters to performance, which is basically the mass flux arrival divided by that 3000 acre feet per year.

Another key point in a system as complex as this for the UZ is that we're not talking about one travel time.

We're talking about a distribution of travel times through the unsaturated zone because of a variety of processes that have been talked about here today.

So, basically, because our goal is to predict distribution of travel times through the UZ, we used a particle tracking model in the TSPA modeling effort in order to achieve the goal.

Now, I'm going to talk about the model formulation for the abstraction model. Basically, this model builds upon the flow and transport modeling that has been presented here today. Basically, the current modeling approach is a dual K or dual permeability model. Our particle tracking model is also a dual K particle tracking model. It's cell based in the sense that particles are routed through the computational grid of the model in proportion to where the water goes. So, where the water goes, the radionuclides go.

Now, they also spend a certain amount of time in each of those computational cells, and that residence time in a particular cell is determined probabilistically, and it's based on a simplified submodel for how we roll up all the complex processes that occur at a scale below the grid cell. If you consider a computational grid cell, it's basically tens of meters by tens of meters, and we have to capture all of the processes that occur at a scale smaller than that in a particle tracking type of approach.

And, what I'm going to go into in a minute is how we do that. Now, we use particle tracking, but associated with those particles, which are just computational points that you send through the system, we associate radionuclide mass with that. So, when they reach the water table, they are then converted back to radionuclide masses.

This is a little more detail on how we handle the, essentially what it amounts to is an upscaling problem, transport at the subgrid scale, we conceptualize as a system of parallel flow in the fractures and matrix, so this little diagram here shows slow in the fracture, parallel flow in the matrix, particles are able to travel either in the fractures, in the matrix, or transfer between fractures and matrix due to advection. That is water movement brings the particles, just like they would bring radionuclides into the matrix, or back into the fractures.

Molecular diffusion, as well, is a process which spreads contaminant from fracture into matrix, or matrix into fracture. And, then for sorption, we use the linear reversible equilibrium sorption model, so-called Kd model for sorption.

I'd like to touch briefly on how this model hooks up with the other models in the TSPA analysis, upstream and downstream of the UZ.

As George pointed out, the unsaturated zone flow is critical to any prediction of transport. What we do in the TSPA model is inherit directly steady state flow fields from the calibrated three dimensional flow model. This is a map of infiltration. Associated with that infiltration and a calibration to available data is a flow field through the unsaturated zone, based on the dual permeability formulation. And, what we do in this model, is use those fluxes directly to send our particles through the system.

Now, the uncertainty in infiltration, and how that plays out in terms of transport has been mentioned. We capture that uncertainty in the TSPA model by using different infiltration scenarios. Different calibrated models can be developed that have different infiltration maps associated with it. We carry those through to the TSPA level by sampling from different infiltration scenarios.

Climate change was talked about this morning. We incorporate climate change in the TSPA model in general, not just for the UZ, but in general, by shifting to a different climate state after a prescribed period of time. When that happens in the TSPA model, the UZ flow model that I'm talking about here shifts to a new steady state flow field when the climate changes.

So, when wetter climate occurs, we shift to a flow field that presumably has, in fact does have more rapid transport. And, so, when the climate changes, the flow field in the TSPA model changes.

Another aspect of climate change that we consider is water table rise. We have some indications to believe that the water table in the past has been higher than the present. We assume that that will be the case in the future by essentially raising the bottom boundary of the UZ abstraction model to account for the fact that the water table probably will be higher under a wetter climate scenario. So, that gives you a shorter travel time through the unsaturated zone before reaching the water table.

Now, that's UZ flow and climate. Now, how does the engineered barrier system's radionuclide releases fit into this? Essentially, we do a lot of simulations of the sort that George presented in which we release radionuclides across the whole repository. But, that's not how we do it in the TSPA model. In the TSPA model, radionuclide releases occur at single grid points, so that if, potentially as small as a single grid point, so that if a simulation calls for a single package to fail, and of course there's always one failure that occurs first in any model, even if several eventually fail, we do those releases at individual grid points, and we also correlate those release rates to the percolation flux, since obviously where there's more water flowing, the TSPA model predicts greater releases. So, we correlate and get that dependency into the TSPA analyses by releasing things at individual grid points.

Radionuclide mass then is added to the UZ transport model as particles with specified radionuclide mass, and it's done in a point source type fashion. Now, if many waste packages fail, then it starts to look like a release across the entire repository after a while.

Now, finally, when mass leaves the UZ, it enters the SZ. The location of that radionuclide mass is identified. We know where particles leave the system. We've been then into essentially these four different quadrants that I've drawn here at the water table.

And, the mass flux versus time, not just in total, but in each of these four quadrants, is fed to the SZ transport model. That model uses point sources within the SZ within these quadrants. Essentially, we're trying to retain some of the spatial variability in these models at the TSPA level.

Onto validation of the abstraction model. We have to show that the model is appropriately handling the processes that we need it to. We do a series of simulations to prove this in one, two and three dimensions. I'm going to walk through those now.

First, in one dimensional transport, we have a single fracture with a connected matrix, and we do simulations using a particle tracking model, and compare that to a different model formulation, which is basically a discrete fracture model in which we actually grid the thing up and do the computation.

These are comparisons for a variety of different diffusion coefficients, ranging from no diffusion, to very high diffusion coefficients. For a case where it's basically all fracture flow, and then I'll provide a case where it's a more even distribution flow between fracture and matrix, in both cases, over a wide range of these diffusion coefficients for non-sorbing tracer, we see adequate to excellent agreement between the particle tracking model and the discrete fracture model. That's an initial test of the model's ability to handle diffusion.

This is an additional set of 1-D calculations, that includes sorption. So, we're going to very high Kd values, and ensuring that the method that we used in the particle tracking to handle sorption in the matrix is adequately handled.

So, we tested the model in one dimension over a wide range of sorption and diffusion parameters, and it compares favorably to the discrete fracture model.

What I've done so far, therefore, is to ensure that it's sort of the building block that you base a more complex two or three dimensional simulation on is adequately represented with the particle tracking technique.

This is a two dimensional simulation, which starts to get into more of the complexity about how the UZ system works. It's got the layering of the Yucca Mountain model, but it's only a two dimensional model, and it's not the TSPA model. I'll get to that in a moment. The releases, as in George's case, are over the entire repository domain, and what I'm showing you is a series of simulations and comparisons to the T2R3D process model, and the abstraction model. The red curves are the process model. The black curves, the abstraction model.

For the no-diffusion case, what we're showing with no diffusion is that the particles are appropriately being routed through the system in a complex flow domain, because we're showing a good match with a totally different numerical technique. With diffusion, the curves, sort of in the middle here, we're confirming that diffusion, when you add it in with all the other flow processes that are occurring in this two dimensional system, agrees also with the process model quite closely.

And, then, for reference, I'm showing a high diffusion case that kind of shows the envelope of how diffusion affects the model results.

Now, on to 3-D, and this is sort of a validation test, but it also launches us into a discussion of how the UZ behaves, and we've seen a lot of that as well today in the previous simulations.

This is, for testing purposes, a release over the entire repository domain, although remember, I did say that in the TSPA model, for the real calculations, so to speak, we're going to do point releases. This shows, for the various infiltration scenarios, a good agreement in three dimensions for the actual three dimensional model. So, a comparison of the process model again with the abstraction model.

The plot, in terms of how the UZ behaves, really shows a large impact of the infiltration uncertainty. And, we've talked about this in previous simulations. The key point here in terms of validation of the model is that the abstraction model, despite the fact that it's particle based, and it's very fast computationally, it's doing a good job over a wide range of infiltration scenarios.

Now, I'm going to move to the transport processes and parameters, starting with colloids, and how those are represented in the abstraction model that's going to be the basis for the total system performance analyses.

What I show here on this plot is a series of breakthrough curves for that same uniform release over the entire repository of a variety of radionuclides, both aqueous and colloidal.

In the TSPA model, we're handling colloids by making some assumptions about how they transport. We assume there is a fraction of the colloid inventory designated with these IF239 for plutonium, and I241 for americium 241, we assume that there is a fraction of colloids that travelled through the unsaturated zone unretarded and with low diffusion. We model these with low diffusion, so essentially, as George was pointing out, you're basically flowing down fractures with no ability for those colloids to diffuse into the matrix.

And, in TSPA, assuming a fraction of those actually travel unretarded, in keeping with sort of some field evidence that there does seem to be a fraction of colloidally bound radionuclides, such as plutonium, at the Nevada Test Site, that do tend to travel so-called anomalously far distances. But, it's a very small fraction in some cases of the total amount of mass that you have there.

So, a key point in looking at these travel times, which are very short, a median time of about 20 years for this colloidal species, a key point is indicated in this note here, and that is that the dose, the impact of that on dose is going to be controlled by things other than just that travel time. You've got the SZ, for example. But, more importantly, how many radionuclides are really going to be attached to particles that travel in this fashion. That is essentially a source or a release rate part of the equation that really is going to control, ultimately, how much of an impact this has on dose.

Now, moving on to the aqueous species and talking a little bit about sorption and dispersion, you've got rock properties that influence transport, such as porosities, and that sort of thing. Those are obtained, and we have those from the process model, so, just as we have the flow fields, we also had the relevant processes that we're talking about here--rock properties that are required in transport, such as porosities.

For sorption, probability, or stochastic distributions of Kd have been developed for all the key radionuclides, and it's segregated on the basis of the three main rock types that are present in the unsaturated zone, devitrified vitric and zeolitic tuffs.

A brief mention of longitudinal dispersivity. It's in the model, but we set it constant because it tends to have a very low sensitivity in any of the calculations we've done. Why is that? Because the distribution of travel times is much more controlled by matrix diffusion effects and where the radionuclides are released. A little bit of longitudinal dispersion over a 300 meter flow distance in the unsaturated zone really doesn't have that much of an added impact on how the radionuclides spread. So, that's the reason dispersivity tends to be unimportant to performance predictions.

And, again, the note that the dose here, this is not a calculation that can in any way be used for dose at this point, until you fold it into the full TSPA model that includes radionuclide releases, SZ transport and biosphere models.

Matrix diffusion is another process. Without matrix diffusion, we showed how colloids move and how any species would move without matrix diffusion. You're dominated by rapid transport through fractures and faults, but when matrix diffusion essentially allows its radionuclides to sample slow moving fluid in the matrix. So, that's what slows down releases. That's why matrix diffusion tends to slow down the releases.

Now, the parameters that influence diffusion in the TSPA model are represented stochastically. Diffusion coefficient, we have laboratory measurements that form the basis of the parameter distribution for diffusion coefficient. But, in addition to those, there are geometric parameters in this model, such as the fracture spacing and aperture, and that's based on a combination of field observations of things like fracture frequency, as well as flow model results, which try to get a handle on things like the fracture porosity based on pneumatic testing, and that sort of thing.

Now, the final aspect of the diffusion issue has been talked about previously, and it's the active fracture model. The fact that not all fractures are assumed to flow, and I wanted to show a little bit more on that.

This schematic kind of shows it. If that was in any way to scale, those flowing fractures are quite a great bit wider spaced than going into the tunnel and counting fractures in the tunnel. So, basically, the active fracture model gives a wider spacing between flowing fractures.

What that does in transport, as it's been seen in the past, is first of all, the AFM for transport is implemented in the TSPA model. So, just to make that point clear, we're incorporating the AFM model into the TSPA analyses as well.

The result of wider fracture spacings, all else being equal, is shorter first arrival times for the fastest moving portion of the radionuclide plume.

Now, in addition to that uncertainty and what that spacing is, there's also a conceptual model uncertainty in terms of how one actually computes the interaction between fracture and matrix, and that's what's depicted on this slide. There's essentially a couple of different ways that you can conceptualize the gradient in concentration between a fracture and matrix.

Our models are dual K models, and, so, if you just take that literally and say that the concentration gradient from fracture to matrix is based on that single grid block, then you've got a concentration difference divided by the fracture spacing. That's how the gradient term is represented in the dual K fracture/matrix interaction model.

But, an alternative is to take a discrete fracture model approach, and really explicitly model that concentration gradient close to the fracture. That's an alternative way to handle the fracture/matrix interaction term. I call this a conceptual model uncertainty. It's a more mathematical conceptual uncertainty than the sorts of things that Alan Flint was talking about, which are true physical, you know, conceptual uncertainties.

But, nevertheless, different ways of computing that term give you different results. Essentially, the dual K model--well, basically, the particle tracking model that we've developed does allow us to test either of those conceptual models, so that's a nice feature of this model, is that we're able to really assess how much this would matter.

And, the bottom line is that the dual K formulation for the fracture/matrix interaction gives you shorter first arrival times for the fastest moving radionuclides. So, these solid curves without the symbols are those for the dual K. With the symbols, that's the discrete fracture formulation.

Now, at longer times, the curves match up and give you the same prediction. So, this is really an early time behavior that's different for the discrete fracture type formulation.

In TSPA, we're using the dual K formulation for two reasons. One, it's a little bit more conservative, and if we have uncertainty in the conceptual model, we could have either propagated both of those models through the system, or just go with the one that's a little more conservative, and, so, we chose the latter. Also, it's consistent with the way the process models that George presented are put together. And, so, we wanted to maintain a consistent train of thought in terms of the assumptions of the model, right through the TSPA level. So, we're using the dual K formulation for those reasons.

So, in conclusion, I didn't get into computational efficiency, but basically, we're modeling, you know, dozens of radionuclides using this particle tracking method. It's a computationally efficient version of the original process model. It uses the UZ flow fields directly. We include climate change. And, for transport, we have dual permeability sorption and matrix diffusion in colloid processes, just like the process model.

So, we tried as hard as we could to really get all of that detail into the TSPA models, and we did that through the model I'm presenting here.

I showed you validation runs in one, two and three D to confirm that the model is acceptable for its use in TSPA. The abstraction model is coupled to the other TSPA models in a way that retains the spatial variability of radionuclide transport. Releases in one area will be different than releases in another area of the repository, and that's included in the TSPA analyses.

As far as the predictions of what this model is going to give us in the TSPA-LA, clearly, there's a wide range of travel times from the abstraction model and the process model, for that matter. Representative times that show up on the plots that I showed, let's talk about the median UZ travel times for present day conditions and the mean infiltration scenario.

Colloid facilitated radionuclides, very rapid. You know, very rapid travel times through the UZ. For the nonsorbing species such as technetium 99, about 6,000 years for the median. And, for strongly sorbing radionuclides, greater than 10,000 years.

But, it's important to point out that future wetter climate conditions will give you shorter travel times, and we will go to those wetter climate conditions in the course of the TSPA analyses.

So, the parameter uncertainties, I mentioned in the flow and transport processes have been quantified and they will be propagated through the TSPA model. And, I showed you a little bit on conceptual model uncertainty for the fracture/matrix interactions, and how that can also be examined with this model.

Thank you.

CERLING: Priscilla?

NELSON: Nelson, Board.

Slides 13 and 19, the figures look exactly alike.

ROBINSON: They are alike. I used the same figure to, in the first one, demonstrate how colloids behave, and the second one, how some of the aqueous species behave. So, if you look at plutonium or cesium sorbing radionuclides, I'm giving you a basis for comparing a strong sorbing radionuclide with one that may be attached to colloids.

NELSON: Okay. Tell me why neptunium 237 is more than one?

ROBINSON: That's basically an artifact of the way the source term was put in. It's in growth. Basically, we have neptunium being put in at the repository as neptunium 237, but you're also getting in growth from the decay chain, and some of those are adding up to more than unity. It's a good point. I didn't explain that.

NELSON: Thanks.

CERLING: Dan?

BULLEN: Bullen, Board.

Maybe just a clarification. Will you go to your first conclusion slide, which is 23, I think?

When you talk about validation, specifically for the validation runs for 1, 2 and 3-D, I understand as a modeler what you want to do to validate, but is this validation also the same type of validation that you need for validation and verification of a code for NQA-1, approval and acceptance by the Nuclear Regulatory Commission?

ROBINSON: We have gone through that process for this computer code, and adhered to QA procedures for that. This validation that I'm referring to specifically is validation of a TSPA abstraction model, and for that, we're obligated to compare favorably to an underlying process model. To carry that a little bit--to carry the chain a little bit further, that process model is obligated to be validated against, you know, available data, and shown to be an adequate representation of reality. So, that's the chain, backwards from the abstraction.

VAN GENUCHTEN: One little question about the steady state flow and the dual K model, and, I've asked it to many other people. If you have steady state flow, how do you get then still an advective component from, let's say, a fracture into matrix, or does that go then more down gradient, and it comes out again, or how does that go?

ROBINSON: Steady state flow does not mean pressure equilibrium between the fracture and matrix. It just means that the flow is obtained as steady state, in which, you know, pressure differences are, you know, have reached a constant non-changing value in a computation. So, you can still have flow going from fracture to matrix, and in fact, this happens in space, that the interfaces between units, when you go through the TSw, if you have a Calico Hills vitric non-welded right below it, you get a rapid transformation of that water from predominantly fracture flow into the matrix.

VAN GENUCHTEN: Thanks.

CERLING: Dave Diodato?

DIODATO: I guess I'll ask if we can get back on time practically if I pass; right?

CERLING: We're doing fine.

DIODATO: Slide 13 then maybe. On the people that have issues with this validation term in terms of model validation, like model testing, I think you can only invalidate models, but that's not what I'm here to talk to you about.

Can you help me to understand this left-hand experiment that you've got going here, the 1-D thing and how these different curves, what's changing with the experimental set and what happens as you increase the diffusivity from 10 to the negative 20 to 10 to the negative 9?

ROBINSON: So, if a discrete model in which mass in the case of the particle tracking model, particles are put into the fracture. Okay? And, if you have no diffusion into the matrix, such as the ten to the minus 20 case, it shoots down the fracture, and arrives very quickly at the outlet.

What we're plotting here are breakthrough curves at the outlet to a mass input at the inlet. Now, as diffusion coefficient increases, you have a matrix sitting there that has a large volume of water compared to what's in the fracture, and, so, as diffusion coefficient increases, essentially matrix diffusion, what's always called matrix diffusion, occurs here, and thereby slowing down the radionuclide, or the particles in this case.

Now, there's a limit to that. If you get to such a high matrix diffusion coefficient that the mass is essentially sampling the entire space, fracture and matrix, it essentially reverts back to an equivalent continuum with matrix-like properties once again.

So, on the left is a continuum model with just a fracture, and no matrix, since you're not allowing the mass to get into the matrix. On the far right, the highest diffusion coefficient, you essentially have a system where it doesn't matter that all the flow is occurring in the fracture, you're still sampling that matrix, and you have essentially an equivalent continuum model with a much higher effective porosity, that is, the porosity of the matrix is what matters then. And, this is just a test over that entire broad range of conditions.

DIODATO: Thanks for helping me to understand that, because I was looking at that. I thought it looks like plus flow on the left practically, all advection, and then you have the diffusion coming in, and then towards the end, it looks more like it's getting back to an advective case, but with retardation kind of added in.

ROBINSON: That's exactly what it is. And, in fact, the effective porosity for the far most right curve is essentially the matrix porosity. The effective porosity for the left most curve is the fracture porosity.

DIODATO: Interesting. Thank you.

ROBINSON: It spans that whole range.

DIODATO: Thank you.

CERLING: Thank you for your comments, and we've got one public speaker, or one member of the public who signed up for comment at the end of the day. That's Tom McGowan.

MCGOWAN: Thank you, Mr. Chairman. Tom McGowan, Las Vegas resident since 1954, and candidate for election as U.S. Senator for the State of Nevada in 2004. That's a downgraded position for me. As a matter of fact, for some of you, it might be a (inaudible). It's been said also, and this is hypothetical, until and unless we've proven otherwise, including your exhaustively demanding studies and work-product, if any, and insight, I should indicate my deep appreciate for all these fabulous presentations today, and I know you've said colloids were a principal, or at least conversations many years ago in the very beginning, so they apparent are still somewhat.

But, this is nerve racking, Mr. Chairman. Do you mind if I smoke, Mr. Chairman?

CERLING: Smoking is not allowed.

MCGOWAN: I think we'll have no smoking. I get your pardon. Thank you very much. You've just established the unequivocal standard of the release of second-hand smoke within these meeting premises here at the NWTRB's Crowne Plaza Mountain, so to speak. And, it took you less than a micro-nan second to do that.

So, how did you arrive at that important scientific conclusions without reviewing all of the relevant technical factors that do or may apply? For example, how long would it take for a second-hand smoke molecule to travel the distance from the smoker to the nearest human receptor, or the farthest, or to all those in between? And, how do you make that determination? Did you rely on Brown's Law for Gaseous Diffusion Within a Closed Container?

But, this meeting premises isn't a perfect vacuum, although some may think it is. But, others think it's an interminable treadmill in precipitous decline toward an ultimate end-state of self and mutual confoundment, and terminal non-viability. I don't know whether there's plenty of money to support this for another several decades.

But rather, and similar to the other Yucca Mountain, it's comprised of a proliferation of fast pathways and infinite densities, naturally-ordered as in a state of variable dynamic flux, evolving in continuum from its inception and to date inclusively and, foreseeably, for the rest of human/geologic time, in both iterations. God forbid. And, none of these will be as dangerous to human elements as toxic radionuclides. And, death is irreversible and few would argue otherwise.

Comes now a series of pertinent questions for those self-evident as securely confined between a welded tuff and a hard place, with the reminder that your federally mandated charter and by-laws cannot require you to respond to technical scientific query from the interested and affected public: to wit--I'm going to go by that, by the way, so I cleaned most of this up.

What's the deadliest toxic radionuclide contained in high level nuclear waste?

What's the total cumulative term of radioactive half-lives with the longest lived and deadliest toxic radionuclide contained in high level nuclear waste?

What's the arbitrarily imposed and federally mandated term of secure containment of high level nuclear waste within an underground repository?

Where's the accurate, complete and invariable four dimensional hydrogeologic map of the underground environment beneath Yucca Mountain regional area and all of Southern Nevada?

Will the deadliest and longest lived toxic radionuclides inevitably be released, mobilized and transported from an underground repository into and throughout the human accessible underground environment and the ambient biosphere?

And, by extrapolation, do you concur with the reasonable conclusion that on naturally ordered axiomatic grounds, it's scientifically and technically impossible to guarantee the safe, secure, human intrusion impervious permanent underground storage of high level nuclear waste, by any combination of natural and artificial means, either at Yucca Mountain, Nevada, or elsewhere nationally, or anywhere on the planet? Some of you may take exception to that. Don't talk all at once. But, you can get on the public record.

Consequently, the underground emplacement of high level nuclear waste constitutes a direct injection of deadly toxic radionuclides into and throughout the human accessible environment, where it's destined to cause the illness and death of thousands of as yet unborn future generations, and ultimately, it's potentially causal of the premature extinction of human consciousness itself. And, these victims will not be aliens from a distant planet or strangers from a foreign land, but rather, irrefutably, they will be our own progeny, for thousands of generations to come, and thereas, we shall have been the purportedly advanced, sophisticated, current generations of Americans self-labelled as having oxymoronically failed ourselves, each other and posterity, in sight of Almighty God.

Therefore, the fundamental crux issue that permeates these meetings and proceedings to date and in projection, isn't about nuclear waste per se, but has a greater significance and enduring impactive consequence, concerns the human capacity to reason, and the question of integrity, notwithstanding the federally mandated mission, and above all, conscience, in sight of a supreme being, on the deeply personal and introspective individual level, as well as on the human universal scale, and there is a historic precedent for that important decision making process, with your indulgence, I'll relate it.

More than 60 years ago, the impeccably uniformed, well-educated, and seemingly innocuous and benign SS Officer, Adolph Eichman, who never personally forced anyone into a concentration camp, a gas chamber, or an oven, but was hundreds of miles distant and temporarily removed from the ghastly scene of man's inhumanity to man, nevertheless dutifully signed the executive order that carried out the unwritten but widely recognized wish of the maniacal fuhrer, which resulted in the inhumane deaths of millions of innocent and defenseless men, woman and children in the heinous gas chambers and ovens in the death camps of Nazi Germany.

But, despite Eichman's protestations of innocence, and the fact that he was simply carrying out order from a higher authority, as you are doing, the International Tribunal at Nurenburg ruled that separation by time and distance from the consequences of his official action, and the carrying out of an immoral order was not a competent legal defense for the crime of mass genocide on an unprecedented scale. And, Adolph Eichman was found guilty, and was hanged by the neck until dead.

And, if you think there's any significance difference, and with all due respect and deference, every single one of you people--I should clean that up, shouldn't I in a nice way, If you think there's any significant difference between the nuclear waste pertinent president and Congress of the United States, the NAS, the DOE, the EPA, the NRC, the NWTRB and Adolph Eichman, and his ethically and immorally bankrupt higher authority, you're quite mistaken.

And, in fact, you will each and all, however posthumously and in absentia, will be held accountable, responsible, and liable for the impactive consequences of your official acts and omissions in the court of universal world opinion, and in sight of Almighty God.

And, I thank you for your time and interest. And, by the way, your mission--you are among the world's leading scientific, psychological, and academic minds of our time. I respect and admire every single one of you, all of you, without exception. Therefore, you are beyond excuses. You know better. You really do. And, it's your ethical and moral duty and responsibility to all mankind to report back to your Congress and tell them the truth, that this can go on for the next 40 years without any meritorious conclusion. The conclusion was known to very first day. It's impossible, and so am I. I don't go away. I'm like a radionuclide colloid. I'll be coming back. However, I've got to go back to the VA (inaudible). I'm the only one that wanted a second helping of it. But, this may be even more effective.

Ladies and Gentlemen, I love you. I will miss you and be with you. I'll give them to your staff here, and they will make sure that it's inserted somewhere, hopefully, in the proceedings of this public record.

Thank you very much.

CERLING: Thank you, as we adjourn until tomorrow.

(Whereupon, the meeting was adjourned, to be concluded on May 10, 2004.)

A P P E N D I X

1. Letter to Dr. Jacob Paz from the Environmental Protection Agency.

2. Written comments by Tom McGowan.